

Is Tenure Tenable?

In the past few months, there have been several occasions on which the principle of tenure for established academics has been challenged, both in the United States and in Britain. In some American colleges, simple financial stringency has led to the abolition of tenured posts. Elsewhere, there have been complaints that political considerations have been used to deprive academics of tenure or to deny tenure to people who would otherwise have been eligible. In January, the dismissal of Dr Bruce Franklin from a tenured post at Stanford University, eventually upheld by a panel of the faculty, became a public issue. More recently, there have been rumblings of discontent with supposed political interference with faculty appointments. In the *Chronicle of Higher Education* (February 14, 1972), Professor George Adams of the University of Wisconsin at Whitewater complains that a tenured colleague of his was fired by the regents of the university even though his continued appointment had been supported by the faculty and the university president. In Britain, the University of Lancaster has been torn for several weeks with dissension about promotions to tenured posts in the English department and students in all but four of the 24 departments at the university have been boycotting lectures. In the nature of things, of course, generalizations are impossible in such a tricky field. At the same time, however, it is high time that universities should decide more clearly than in the past just what they hope to gain from the principle of tenure, and in particular that they should make up their minds just how essential to the cause of academic freedom the principle of tenure may be. There is at least a chance that the over-zealous defence of the principle may impede the more important task of making sure that universities are equipped to defend what really matters.

The classical concept of the university is that of an autonomous community of scholars, responsible only to itself for the maintenance of academic standards and for the recruitment of new scholars to its midst. In such a setting, it is only proper that new recruits should serve a probationary period before being given what amounts virtually to lifelong job security. It is understandable that the decisions which are eventually reached should often appear arbitrary and that many of those who are denied permanent appointments at universities should feel aggrieved. Not merely are there bound to be occasions when university faculties are less judicious than they should be in making choices, but considerations other than academic ability must frequently play an obvious part in the making of decisions. After all, if universities are to be regarded as self-governing institutions, it is inevitable and even desirable that the prejudices of the established faculty should come into play. If a Catholic university is prejudiced against non-Catholics, or an African university is prejudiced against Europeans, few people will protest. Evidently there must be more serious questions to contend with when political prejudices appear to be dominant, but in principle at least there is no reason why an intellectually independent university

should not make decisions on such grounds. To deny them this right would in itself be an infringement of academic freedom.

Unhappily, as the whole world knows, the procedures at present used by most universities for confirming academics in permanent appointments are neither systematic enough nor public enough to carry conviction. In most English-speaking universities, proposals for appointments to tenured posts are made by faculty committees and afterwards confirmed by the governing body of the university. In most circumstances, it is inevitable that the recommendations of departmental heads will be accepted by other members of the faculty. If a candidate for promotion to a tenured post can be demonstrated to have established an academic reputation for himself, and if his established colleagues fear that to deny promotion would be to invite a potentially useful colleague to find a job elsewhere, the chances are that they will listen sympathetically to a proposal that he (or she) should be given a permanent appointment. Although there may be circumstances in which this essentially informal selection procedure may be inequitably applied, and even though the need for rapid expansion may sometimes mean that undeserving people are promoted too quickly, there is at least a semblance of democracy in the way in which the system works.

The difficulty is that the way in which promotions to tenured posts are decided is no guarantee that recruits to the academic community will strengthen its capacity to work out a constructive academic policy and to ensure that future recruits will be able to perpetuate that tradition. At the most banal level, it must be acknowledged that some academics go intellectually to sleep long before they qualify for their old age pensions. More seriously, there are few universities in which established academics are equally engaged in making academic policy, for even at Oxford and Cambridge the departmental structure is, quite understandably, an increasing obstacle to free decision-making.

What can be done to strengthen these procedures? Now that the faculties of universities are counted in thousands and not hundreds, it is clearly impossible to expect that the endless round of faculty committee meetings on which academics spend an increasing proportion of their waking hours can be effective in making academic policy. For one thing, there can be no certainty that the accumulation of decisions on the particular matters with which the committees deal will constitute a coherent and positive policy. How, for example, is a university to decide that it will try to contribute more directly to the solution of important social problems (which students and faculty alike are often asking) without some forum within which all members of the faculty can have their say? But there are also important problems of a practical character which cannot be dealt with corporately under the machinery which now exists. If, for example, economic pressures require that some departments should be closed, or if new opportunities for



development present themselves, it is exceedingly difficult with present arrangements to ensure that the proposed community of scholars will function as a whole. At times of stress, the administration has the whip hand. Where new developments are possible, universities are frequently too uncritical of the projects which enthusiasts would force upon them.

It follows that universities need better methods for constructing academic policy which is in some sense corporate. The problem is identical with that of constructing a government under a parliamentary system, which usually entails some form of election for a cabinet much smaller than the body of those who feel they have a right to an opinion. In universities, however, such a procedure would entail not merely that academics who had let their reputations rust would be neglected but also that established academics of all ages would often find themselves at loggerheads with what their universities were doing. It is a simple corollary to conclude that if universities were efficiently governed by their academic communities, the principle of tenure would often entail contradictions that could not indefinitely be stomached. In this sense, tenure is not so much an assurance of academic freedom as an impediment to good government.

So why should not the system of academic tenure be abandoned? University teachers are often unaware of how remarkable is the privilege of tenure which they enjoy at present. Even in the British Civil Service, there are now devices by means of which inefficient performers can be eased out. The notion that men and women can be appointed to university posts with an implicit assurance that they will remain on the payroll until they retire is as bad for the people concerned as for the universities in which they work. To be sure, nobody would wish that academic work should be turned into a precarious employment, but is there any reason why a five or seven year period of engagement would not meet the need for personal security, for continuity and for academic freedom? Especially if appointments were made and remade corporately, it is hard to think that such a system would deal unjustly with those who had made valuable contributions to the development of a university or who had won reputations as teachers or researchers. The benefits would be that universities might become

more flexible, that all who work in them would have an incentive not finally to lock themselves away in ivory towers and that the universities as a whole would be able to earn a better reputation for efficiency and effective self-government. The abuses of the tenure system, and in particular the way in which it allows some departments permanently to go to sleep, are a force in the other direction.

What would become, in circumstances like these, of complaints of political interference with academic promotion? In most of the arguments which there have been in recent months about political interference with academic appointments, universities have been dangerously indecisive. Whether or not supported by public funds, universities are bound to be a little like self-perpetuating clubs. For these reasons, it is entirely proper that an academic community should ask, when electing a newcomer to its strength, that he should be acceptable not merely on grounds of academic reputation or as a teacher but for his manner and even for his opinions on the function of the university. To be sure, universities would quickly destroy themselves if they were narrow-minded in the application of these criteria. A university which turned its face against communists, for example, would quickly become a laughing stock. But it is proper for a university to ask of new recruits that they should not set out to oppose the policy of the university government on matters such as relations with students or the extent to which courses of study in apparently academic subjects should be made vehicles for political activity. Here again, of course, wise universities must be liberal, recognizing that times have changed and that there are great benefits, pedagogical and social, in making courses of study more relevant to the real world. But in the last resort, the university has a duty as well as a right to say what it expects of those who teach. In recent months, there has been serious confusion about the true meaning of academic freedom. The essential need is that universities should be corporately immune from pressure from outside. It is less important that individual academics should be free to do as they wish within the institutions which employ them. They, after all, are always free to go elsewhere. To the extent that the tenure system has helped to encourage this confusion, that is another case for thinking that it should be changed.

Conserving Species

THE British Government has done the decent thing by tigers, snow leopards and clouded leopards. On March 27, the Department of Trade and Industry announced that it would in future ban the import into Britain of furs, skins, rugs and coverlets made from the skins of these dwindling species. The department has been less sensible about leopards in general as well as cheetahs, for which it is now laid down that skins can be imported only if they are accompanied by certificates to demonstrate that the animals concerned have been killed legally and that their skins can be exported from the countries in which they live. Regrettably, the Department of Trade and Industry has not yet summoned up the courage to

take a similar line on other species whose survival is threatened not merely by changes in their natural environment but, more acutely, by the activities of hunters. What, for example, of polar bears? Why should not the British Government follow that of the United States by declaring a ban on the import of whale products of all kinds? And it is earnestly to be hoped that the steps which have been taken in Britain to protect tigers and the more exotic leopards should not be regarded as a sufficient assurance of survival for the species concerned. Given that the demand for tiger skins by the fur trade has been greater than the supply for several years, the chances are that the skins produced by hunters, authorized or illicit, will

find a ready market in countries which do not operate a ban on imports.

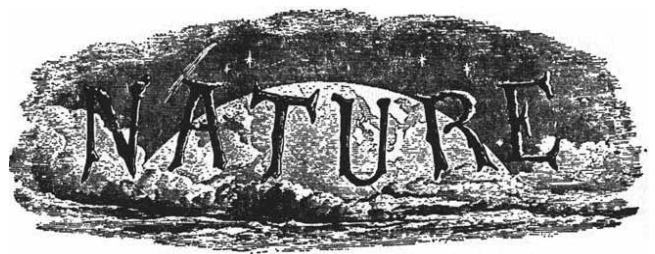
In present circumstances, there is no justification for the continued slaughter of these animals. The days have gone when tigers in India were a threat to the rural population of human beings and, since then, shooting tigers has become a minority sport to say the best of it and one of several sources of raw material for the clothing industry. There is no reason to suppose that a cost benefit analysis of the hunting business, if properly conducted, would lead to the conclusion that the sport should carry on. To be sure, hunters often bring with them benefits of an unexpected kind for the communities in which they indulge their sport. There may be beaters to be hired, ammunition to be bought, guns to be carried and hotel rooms to be hired. In the long run, however, the communities that are fortunate to have stocks of rare species nearby can expect to do better from those who come with cameras and not guns. Indeed, by allowing hunting now, they may deprive themselves of much more substantial benefits in the years ahead. And there are few people who value the availability of exotic furs in the fashion business before the equally intangible advantages of the continued existence of tigers, leopards, bears and elephants, only a few of the species whose survival has been cast in doubt by hunting for the sake of hunting. It goes without saying, of course, that much the same is true of the way in which armies of civilians spend much of the autumn blazing away with shotguns at birds such as grouse and geese. It has been estimated that the annual toll of game birds in North America exceeds two million—a much greater external pressure on the bird population than any recent change in the natural environment. It follows that there must be a long way to go before devices such as the British Government's ban on imported tiger skins will amount to a policy for the conservation of endangered species.

In the long run, it will be for the governments of the territories which these species inhabit to take effective action. In recent years, there has been a welcome awareness in Africa and elsewhere of the need for conservation. The experience of East Africa has been especially valuable. With changing circumstances and at times when the territory available for particular species is declining, it may be necessary to keep numbers under control by carefully regulated hunting. This, for example, has been found necessary as a means of keeping elephants in check in many parts of Africa. It goes without saying, however, that such a policy must be based on a sensitive understanding of the size and age structure of the population concerned and of its ecological relationship with other species in the same territory. Although it has sometimes been thought that systematic control of wild populations can be combined with a limited amount of hunting, the chances are that this will turn out to be an illusion. And in any case, for most species, the problem is not an excess of numbers but a shortage. This is why it is to be hoped that devices such as the British ban on the import of tiger skins will be regarded chiefly as preliminaries to putting pressure on the governments which still allow hunting or which operate imperfectly monitored restrictions.

In many circumstances, international action will be needed. The several species of whales which inhabit the South Atlantic are unhappily a vivid illustration of how difficult it is to devise workable agreements for the preservation of rare species. Although the blue whale has

not been quite exterminated, so that the few hundreds of these animals which still survive may yet be able to produce a large and viable population, it is clear that the International Whaling Commission which is supposed to regulate the whaling industry has not been able to adopt and to enforce sufficiently rigorous restrictions. The difficulty has always been that countries which are attached to the whaling industry, Japan and the Soviet Union, for example, seem always successfully to be able to resist the pressure for conservation. Like hunting, however, whaling is an activity with few tangible economic benefits. The whaling industry is worth no more than £10 million a year, and its products (such as whalebone) can be substituted by other materials and are, in any case, only marginally cheaper than the alternatives. So is there not a case for asking that the conservation of whales should become an important issue in international relations? If nations are able to inhibit others from external activities often regarded as of crucial importance, surely they must be able to force through the United Nations a total ban on the hunting of the whale. And, that done, it should be possible to set about improving the arrangements which at present regulate the hunting for fish, many of them of far greater commercial importance than are the whales. In the past few years, the North-West Atlantic Fisheries Commission has done some useful work helping to define the limitations which should be observed by those who fish for humble creatures such as cod and haddock, but there is always a danger that when natural populations are seriously endangered, recalcitrant fishing nations will put their immediate interests first.

100 Years Ago



The Adamites

I SHOULD not have noticed the letter of "M. A. I.," which appeared in the last number of *NATURE*, with reference to my paper on "The Adamites," were it not that my silence might be interpreted as an acknowledgment of the justice of the remarks of the anonymous writer. If I had been silent, however, I trust your readers would have had more sense than to accept the dictum of a writer, anonymous or otherwise, who thinks to negative the conclusions of a paper, written at least in a truly scientific spirit, by such nonsense as the reference to *Paddy* and *Taffy*. One looks for reasoning in the criticisms which appear in such a journal as *NATURE*, and not for a misleading statement of an opponent's position, supported by reference to general conclusions and the use of weak satire. When "M. A. I." condescends to advance an argument, I shall be happy to consider it; and if it should be unanswerable, I shall not hesitate to admit it to be so. Doubtless I ought to feel thankful for the tenderness with which he has trodden on my toes, but I have scant regard for mere courtesy where questions of science are at stake; and in the interests of truth I would rather that the errors of my "unlucky paper" should be openly exposed, than that I should be "damned with faint praise."

Hull, April 8

C. STANILAND WAKE

From *Nature*, 5, 460, April 11, 1872.

OLD WORLD

Critical Time for Nuclear Power

ALTHOUGH the Vinter report on the future of Britain's nuclear power programme has been completed, it will probably not, in spite of frenzied speculation about its contents, see the light of day. The report—officially known as the Thermal Reactor Review—is the result of a departmental inquiry begun in the distant days of October 1970 and is intended to advise ministers on which type of reactor Britain should plump for to fill the gap between the advanced gas cooled reactors (AGR), which are not yet operational, and the breeder reactors which are expected to start generating power in the 1980s.

The report will not be made public because it is a departmental inquiry, and such inquiries are not normally published, further, the report examines in depth the pros and cons of competing reactor types, and no doubt contains details of the systems which the companies concerned would not want made public. Nevertheless a decision which could affect Britain's nuclear power programme for the next ten years will be based on the report, and it is to be hoped that, once the decision is announced, ministers will provide justification for their choice.

But that choice is no easy one, for the alternatives that confront ministers are myriad. Three British and two American reactor systems were studied by Mr Peter Vinter and his committee. The British ones are the AGR, the steam generating heavy water reactor (SGHWR), and the high temperature reactor (HTR) which, like the AGR, is gas cooled and is the next stage in this type of reactor. Of these the SGHWR and HTR both still need many millions of pounds of research, and the AGR looks as though it may be plagued with corrosion problems as were the first Magnox reactors produced by Britain.

The American options are both light water reactors—the boiling water (BWR) and pressurized water reactors (PWR) which have sold well around the world; in fact, ninety per cent of nuclear power stations outside Britain are BWRs or PWRs. There have, however, been hotly disputed question marks over their safety, and if Britain were to buy the licence to build them from Westinghouse it would mean throwing the long term experience gained from almost £2,000 million of gas cooled technology down the drain.

Against this must be set the potential economic benefits of being able to sell

the most popular nuclear reactors to the rest of the world—a sales exercise worth £1,500 million over the next ten years according to a private report circulated by the British Nuclear Forum recently. In the light of Britain's inability to date to sell any AGRs or SGHWRs in foreign markets, this is quite a carrot to hang in front of any minister's nose. There is little doubt that Britain's nuclear power industry, if it had to, could cope with the change, although it would have a lot of homework to do to catch up. The real question, if Britain abandons gas technology, is whether there will be enough time left to make a real profit out of water reactors before the fast breeders—which will be cheaper in the long term than thermal reactors—come on to the market.

It is clear that since Vinter started

work the arguments for the various systems have proved to be much more evenly balanced than was previously thought. The suggestion of buying American reactors would have produced universal cries of horror eighteen months ago, but whichever system is chosen it is important that the decision be reached soon.

But vital though the decision is to the immediate future of Britain's nuclear power, it can be seen as only a stopgap move until the fast breeder reactor—the prototype of which is now nearing completion at Dounreay—becomes a commercial proposition. At the moment Britain has a world lead in the fast breeder reactors, and it might be wise to see that the research—and the development—is done to ensure that the breeder reactor becomes a commercial proposition.

COMPUTERS

Three into One

ONLY five months after the Select Committee on Science and Technology recommended a rationalization of the way in which the British Government purchases computers, the government has acted by setting up a Central Computer Agency, which came into existence on April 1 (see *Nature*, 234, 117; 1971). The new agency is part of the Civil Service Department and it will centralize computer buying activities at present spread among the Department of Trade and Industry, the Stationery Office and the Civil Service Department.

The new agency will eventually assimilate 600 civil servants, of which 400 are at present employed by the Stationery Office, 100 by the Technical Support Unit of the DTI and 100 by the Civil Service Department. In spite of the fact that the agency is to form a part of the CSD, the overall sponsorship of the computer industry in Britain is to remain in the hands of the DTI.

The director of the new body is Mr Stephen Spain, who was previously an under-secretary at the Civil Service Department. The deputy director has yet to be appointed, but a spokesman for the CSD said last week that his department was looking for someone from outside the Civil Service who would be appointed on a contract basis. As part of its new outlook on computers the government is also planning to second two of its senior computer

personnel to outside organizations.

The agency will be responsible for a budget which at present runs at £30 million a year. At the end of 1971 government departments had 213 computers, of which 93 were used for scientific work; the remainder were programmed to carry out administrative tasks. Mr Spain's job will be to coordinate the activities of government in policy making, procurement and operational support, but backing for the computer hardware industry in general will remain under the aegis of the DTI.

There has been no government decision yet about a further recommendation of the Select Committee that a computer research and development board should be set up. The Select Committee suggested that this board would decide on the apportionment of support to different British industries. Also the government has not, as yet, succumbed to the entreaties of the Select Committee that it should pump £50 million a year into the computer industry—this sum is to be compared with the £3.2 million which the government has provided in the past six years.

The government's intentions will be made known soon with the publication of the long awaited white paper on the computer industry. There seems little doubt that the benevolence of the government will not extend to £50 million a year, but informed sources think that £20 to £36 million might be nearer the mark.

ELECTRICITY

Power for Ireland

from a Correspondent

THE forecasts of 9 per cent annual growth in electricity demand in the Republic of Ireland and a 90 per cent reliance on imported fuel oil by the end of the decade were the motivating factors behind recent studies commissioned by the Electricity Supply Board (ESB). A re-evaluation of present policy leading to a nuclear energy programme was contained in two papers given recently in Dublin before joint meetings of the Institution of Electrical Engineers and the Institution of Engineers of Ireland by Mr Sean Tinney, director of ESB Generating-Transmission and two members of the ESB project department.

To cope with an expected demand for some 3,000 MW by 1980, the ESB report advocates commissioning Ireland's first nuclear power station of 500 MW capacity at a cost of £90 million. A final decision on the timetable for nuclear power is not expected before June 1973 when the results of exploratory drilling for natural gas some 27 miles off the coast of Cork will be known.

The ESB reports present a cost analysis of current and future nuclear technologies within the reference frame of Irish national policy on electricity. They lay stress on cheap and reliable power together with a concern for public health and safety. While the initial cost of the nuclear plant (twice that of a comparable oil-fired installation) presents problems for a capital starved country like Ireland, the low fuel costs will compensate to some extent.

Security of fuel supply has become a major concern in Ireland with the complete reversal by the end of the decade of the situation prevailing in 1935 when native resources supplied 80 per cent of Ireland's electricity. Wartime restrictions on imported fossil fuels are still remembered in Ireland and nuclear fuel, with its cheapness and ease of storage, is an attractive proposition. At present only the USA sells enriched uranium in quantity, but by 1980 this bottleneck will be removed when a European gas diffusion plant becomes operational. The ESB engineers estimate that a nuclear fuel cell fabrication plant employing 200 men could be built locally for a capital outlay of only £1 million.

The ESB project report narrowed the technological choice for Eire's first nuclear station between the two standard light water reactors: the boiling water system (BWR), using a direct cycle with steam fed directly into the turbine and the pressurized water system (PWR), with an indirect cycle with steam generators interposed be-

tween primary core coolant cycle and secondary steam cycle.

At the meetings, the ESB economic analysis came under some criticism for failing to estimate the concomitant social costs of bringing nuclear technology to Ireland over and above the £90 million for the hardware. Also questioned was a lack of ESB consideration of a nearby pumped storage scheme to utilize fully the off-peak load from the nuclear station. The two case-studies, however, were felt to be important contributions to the problem of bringing nuclear power to an under-developed country.

DRUGS

Modest Proposals

from a Correspondent

A MORE reliable system for compiling data on the use and abuse of drugs was recommended by the Council of Europe's multidisciplinary conference on drug abuse, held recently in Strasbourg.

Dr Anthony May, regional director of mental health for the European division of the World Health Organization, impressed upon the conference the need for standard criteria and terminology to describe drug addiction. At present countries set their own standards based upon clinical impressions, and evidence from administrative, social and non-medical sources, and consideration is also given to the self-reporting of drug addicts themselves. The information obtained from this last source is questionable at the best of times and, in view of the fact that simple tests for type and quantity of drugs taken are not available, attempts to unravel the

history of addiction have in the past been woefully inadequate.

Dr May disagreed with the view that the use of drugs and the alienation of youth are the products of a social malaise which can only be corrected by a radical reorganization of society. He also said that the prevalence of drug addiction could not be explained in either pharmacological or psychological terms alone. Dr May considered that research into addiction should be carried out on a broad epidemiological basis that integrated all aspects of the problem.

Dr May's approach met with some criticism from the other delegates, but he countered by saying that the inclusion of family background, school and other environmental factors would enable the scientist to study the incidence of drug taking over long periods of time. In this way half truths and half myths—for example the feeling that 90 per cent of cannabis smokers "grow out of it" at twenty—could be scientifically examined.

In spite of the Council of Europe's recommendations that it should set up its own centres to investigate drug addiction it seems likely that the World Health Organization will carry out most of the research in its research centres in European countries. Dr May stressed, however, that WHO did not wish to usurp the Council of Europe's initiative and that they could, in fact, work very well together. Dr May said that WHO could organize the research and that the Council "which is very well organized for the dissemination of information" could inform the world of the results. "I can think of no reason," said Dr May, "why this should not be a very happy marriage."

RESEARCH ASSOCIATIONS

More Scrutiny

HARD on the heels of a report by the Centre for the Study of Industrial Innovation on the role of the research associations (see *Nature*, 236, 136; 1972) comes an announcement by the Conference of Industrial Research Associations that an inquiry is to be held to "examine the scope, organization, functions and methods of operation" of the associations. The inquiry is to be headed by Lord Bessborough, and will also include Lord Energlyn, Dr T. Emmerson, Dr F. E. Jones and Sir Harry Melville; it will examine, in particular, the relationship and interaction between the associations and government, industry and universities, with the object "of making suggestions to increase their industrial effectiveness".

Government aid for the research

associations has been steadily reduced since 1967-68, and the associations are under some pressure to make themselves less dependent on grants. The inquiry will be welcomed, if only because it should allow the associations to see clearly where their future lies.

The table shows the sources of finance of the research associations during 1969-70—the latest year for which figures are available.

Sources of Finance of the Research Associations 1969-70

	£ million	%
Government	5.31	32.3
Public corporations	0.78	54.8
Private industry	8.82	53.6
Overseas	1.29	7.5
Research associa- tions*	0.24	1.4
Total	16.45	

*Sub-contracted work.

A Decade of Russian Space Achievement

from our Soviet Correspondent

MARCH 16, 1972, was the tenth anniversary of the most prolonged of all Soviet space projects to date—the Kosmos series of satellites. This series, which has covered a wide range of projects—from precursors of manned and deep-space automatic missions to apparently military or paramilitary projects—has totalled almost five hundred launches in its ten years of operation, and has become so commonplace that, except in certain special cases, the western press has long since lost interest in Kosmos launches.

The Kosmos programme started modestly enough, with a TASS report, published on March 16, 1962, under the headline "Satellite in flight". A satellite, it was stated, had been launched, as part of a programme of space research, to be carried out "in the course of 1962", which would include:

- study of the concentration of charged particles in the ionosphere with the aim of investigating the properties of radio waves;
- study of the energy states of the radiation belts of the Earth with the aim of estimating the radiation hazards for long-term space flights;
- study of cosmic rays and variations of their intensity;
- study of the magnetic field of the Earth;
- study of short-wave radiation of the Sun;
- study of upper layers of the atmosphere;
- study of the action of meteor matter on space objects;
- study of the distribution and formation of cloud systems in the Earth's atmosphere.

Oddly enough, the name Kosmos-1 was applied to this satellite only retrospectively, when, on April 7 of the same year, TASS reported the launch, on the previous day, of Kosmos-2. Now, ten years after its inauguration, the Kosmos programme has become so regular a feature of the Soviet press that it requires something out of the ordinary to make a Kosmos launch more than a routine matter; as, for example, in the case of the first launch of eight Kosmos satellites from a single carrier (Kosmos-336-343) in April 1970, nicely timed to coincide with the centenary celebrations of Lenin's birth.

Kosmos launches are normally reported in a formulaic manner which

gives little more than a statement of initial period, perigee, apogee and inclination, possibly the frequency of radio transmissions, and the note that, in addition to scientific instruments, the on-board equipment includes a radio system for measuring orbital elements and a telemetry system and that "all systems are working normally".

Occasionally this formula has given rise to considerable speculation abroad; such an announcement was made in the case of Kosmos-57 which was launched on February 22, 1965, and which blew up, or was blown up, the same day into more than 150 fragments but which was reported by TASS the following day to have "all systems functioning normally". As early as Kosmos-22 (November 16, 1963), certain Kosmos satellites have been observed to manoeuvre in orbit, but no mention of this has been made in the Soviet press. Kosmos-146, with a length of approximately 14 metres, was observed to have arrived in orbit on March 10, 1967, and after the first day contracted to approximately 9 metres by emitting two capsules. This satellite was generally assumed to have been a Soyuz precursor.

The Kosmos label has, almost certainly, covered a number of precursors and failures. Kosmos-111, for example, is suspected to have been a failed lunar probe and Kosmos-41 a rehearsal for the Molniya communications satellites. Certain groups of Kosmos satellites, known as 8-day satellites (because they re-enter precisely 8 days after launch), which do not transmit telemetered data and which are not subsequently reported in the Soviet scientific literature, are widely thought to be some type of paramilitary reconnaissance satellites. (It will be remembered that Kosmos-5 was announced to have monitored the radiation of the US Starfish atomic test, which coincided with the opening of a disarmament conference in Moscow.)

On the tenth anniversary of the programme, however, it is perhaps more appropriate to recall its successes—the spectacular automatic docking of Kosmos-186 (launched October 27, 1967) and Kosmos-188 (launched October 30, 1967). (Kosmos-187 was launched the same day as 186, but for some reason this was not used for the link-up.) Automatic docking and undocking of the pair controlled from Earth was achieved on October 30, 1967, and was headlined as "A new space achievement on the very eve of [the fiftieth anniversary of] the Great October [Revolution]." This was far more than a propaganda achievement.

Similarly, but on the manned side of the project, the flight of Kosmos-110

(February–March 1966) was a necessary step in the Tsiolkovskii-type orbital space station (later developed in Salyut). At the time of the launch, Academician V. V. Parin affirmed that every new "step" in space which involved human participation would be preceded by biological experiments. The dogs of Kosmos-110 were to test the effect of prolonged weightlessness and radiation exposure; they were purposely orbited very high in the Van Allen radiation belts, and as the Russians then were greatly interested in the development of an anti-radiation serum, it is highly possible that the administration of such a serum to one of the dogs was a prime purpose of the mission. After the inauguration of the Soyuz missions in 1967, however, there was an apparent long lull in biological experiments. In October 1970, however, there was a return to this theme with Kosmos-368, which carried "scientific apparatus for testing experimental life support systems for laboratory animals, for the further study of space flight on living organisms, and for the continuation of the investigation of the physical characteristics of space".

It is probable, in effect, that many of the Kosmos experiments are seen as the early stages of research for the relatively distant future. In 1965, when Kosmos 80-84 were launched from the same rocket (the first quintuple launch) it was stated that "the on-board system of power supply of one of the satellites is a power unit operating on power supplied by a radioactive isotope". Soviet space probes normally use solar batteries (indeed, some communications failures seem to have been due to a failure in alignment of the solar cells). Was the relevant Kosmos of this group perhaps testing out equipment to be used for some future deep-space probe, to distances where solar power would no longer be effective?

Another achievement was Kosmos-243, which carried out the first "global experiment on the reception of thermal radio-emission of the Earth and its atmosphere", permitting, in particular, the mapping of the Antarctic ice, irrespective of whether or not it was concealed by cloud-cover. Interviewed in *Pravda*, in honour of the tenth anniversary of the series, Academicians S. N. Vernov (Institute of Atomic Research, Moscow University), A. M. Obukhov (Institute of Atmosphere Physics of the Soviet Academy of Sciences) and Dr Yu. G. Shafer (Institute of Space Physics of the Yakutsk Section of the Siberian Branch of the Soviet Academy of Sciences) stressed advances of this type rather than the occasional "spectacular".

NEW WORLD

Rocky Road to Stockholm

from our New York Correspondent

FINAL plans for the United Nations Conference on the Human Environment, in Stockholm, were announced last week in New York by Maurice Strong, Secretary-General of the conference (see *Nature*, January 1, 1971). After four years of confusion, disagreement, and diplomatic manoeuvring, it appears that Strong has managed to put together a conference that will at least give the semblance of world concern over environmental issues and that might even lead to international agreement and action. President Nixon for one seems to be aware of the political possibilities of the meeting. He has already pledged United States support for a future environmental organization based on the United Nations, and rumour has it that he is planning a visit to Stockholm.

"The environment issue is moving out of the 'motherhood' stage," Strong declared last week, "to the point where it is now being seen as one of the most pervasive, profound and revolutionary issues that man has ever faced." Strong's planning staff and the 27-nation Preparatory Committee have put together over the past two years a 600-page set of proposals, combining rhetoric and action, to be formally considered by the 100-odd nations attending the conference.

For the past week the Preparatory Committee has been thrashing out the text of some 23 principles that make up a general Declaration on the Human Environment. "This will constitute the first attempt by the nations of the world," said Strong, "to agree on the new principles of international behaviour and responsibility on which effective management of the global environment must be based."

The broadest area for consideration, however, is the Action Plan, a compendium of specific proposals and recommendations, including proposals for an "Earthwatch" system of global monitoring and information exchange. Altogether there are some 200 recommendations for international action which the conference is supposed to endorse or reject. An additional 300 recommendations for national-level action are listed as suggestions. Many of these proposals have been submitted by individual countries, such as the American-proposed ocean dumping treaty, and many are the results of political bargaining and compromise. "The Action Plan must be seen as a key

element in a political process," Strong emphasized last week, "the indispensable process of achieving the degree of international political consensus without which no meaningful action could proceed."

One of the six sections of the Action Plan is entitled "Development and Environment" and grew out of a planning conference last June at Founex, Switzerland. The resulting Founex Report attempted to place environmental issues within the framework of development problems. It also pointed out the adverse international trade implications of environmental actions that might be taken by the industrial nations, including restrictions on the importation and use of certain commodities, recycling raw materials and a decrease in economic assistance. "If the concern for human environment reinforces the commitment to development," the report noted, "it must also reinforce the commitment to international aid. . . . Unless appropriate economic action is taken, there are a number of ways in which the developing countries could suffer rather than profit from the new emphasis on environment." Many of the proposals do attempt to reassure the developing countries, and the declaration includes the principle that nations have the right to exploit their own resources, and that the environmental policies of all countries should enhance, not adversely affect, the development potential of developing countries.

The poor countries, and especially Brazil, have not been totally convinced by all this rhetoric, but for the moment it looks as though they will attend the conference. This is a good thing for Strong, for Russia and Czechoslovakia announced a few weeks ago that they would not attend if East Germany is excluded. East Germany and West Germany are not members of the United Nations, but East Germany, unlike West Germany, is also not a member of any of the associated international agencies. So West Germany is attending the conference, but unless East Germany is elected to one of the international organizations, such as the World Health Organization, before June, Eastern European participation in the conference might well be limited or non-existent.

While it would obviously lessen the impact of the conference if a major bloc of nations does not participate, the long-term results really will not come into play until next autumn when

the General Assembly meets and, hopefully, establishes a permanent body within the United Nations to deal with international environmental issues. Agreement for the need for such a body and a means of funding it probably will be the most important items to come out of the conference. Strong stated at the last planning meeting that such a body would need from \$30 to \$40 million a year to operate; President Nixon last week claimed that the United States would be prepared to provide \$100 million over five years. It is expected that the United Nations will propose Strong as head of the new organization.

Strong has enough problems facing him within the conference, but he has also been attempting to deal with even more dissident groups outside the formal conference structure. Many of the international youth organizations have criticized the conference for avoiding what they see as the basic issues of the day—the Vietnam war, and American war crimes and chemical-biological warfare. There was fear that these groups would attempt to disrupt the conference, so at last summer's International Youth Conference on the Human Environment in Hamilton, Ontario, Strong agreed to allow 12 delegates from the Hamilton conference to attend as observers working with the International Non-Governmental Organizations (NGOs), and to set up an Environmental Forum.

The Forum would provide a place for all non-participants in the conference itself to meet and hold their own programmes. A working committee of the NGOs, headed by Walter Bogan of the Scientists' Institute for Public Information, is organizing the Forum. Most of the groups have, as yet, failed to suggest firm programmes, but the committee, working with the Swedish NGOs—the United Nations Association of Sweden and the National Council of Swedish Youth—have managed to get the site of the Forum changed from Stockholm's Tennis Stadium, an obviously inadequate site that would have isolated the Forum from the conference, to the more centrally located National School of Arts, Cinema House, and National Park. It remains to be seen if, between now and June, the NGOs can make use of these spaces to pull together a reasonable counter-conference. Whether they can or not, they are certainly likely to make Stockholm more lively in June.

DEEP SEA DRILLING

Three Year Extension

by our Washington Correspondent

THE National Science Foundation's highly successful Deep Sea Drilling Project (DSDP) has been given a new lease of life. The foundation announced last week that the DSDP drilling ship, *Glomar Challenger*, will continue drilling into ocean floors until August 1975, and that it will take samples from previously uncharted areas. The DSDP was originally scheduled to finish later this year. The extension of the project will enable another 19 voyages to be made, at a cost of \$33 million, bringing the total cost of the project up to \$68 million.

The decision is a massive vote of confidence in a project which has already been instrumental in shaping or confirming theories of geophysics which have in the past few years revolutionized concepts about the history of the Earth. Since its first voyage in 1968, the *Glomar Challenger* has drilled in 220 separate locations, sending back a wealth of information about the constitution and age of ocean floors. Among other things, this information has tended to confirm the theory of sea floor spreading, since it has demonstrated that the age of the ocean floor increases from the area of crustal regeneration at the mid-ocean ridges to the deep sea trenches where it is destroyed.

The extension of the Deep Sea Drilling Project will take the *Glomar Challenger* into Arctic and Antarctic waters for the first time, and it will also take samples from the Red Sea floor, where the enigmatic areas of hot brine and mineral sediments will be studied. Also planned is a drill which will go up to three thousand feet into the rock beneath the sediments of the ocean. Previous drills have only gone a hundred feet or so beneath the sediments.

The voyage into the Red Sea should help to pin down the rate of spread of the sea floor in that area, which in turn will give the speed at which Africa and Arabia are moving apart. This area is particularly interesting scientifically, since the Red Sea is one of the youngest oceans in the world, but it is also interesting because there are layers of brine at the base of the Red Sea which are much hotter than the surface waters. One theory is that these patches of brine are rich in minerals derived from geological process rather than by recirculation, and there has been considerable interest in commercial exploitation of the minerals in the brine and in the bottom sediments. For one thing, since the Red Sea is relatively shallow, mineral deposits are fairly easily accessible.

As for the proposed drillings in the Arctic and Antarctic, one likely consequence is that the date at which Australia broke away from Gondwanaland—a variable about which there has been considerable speculation and disagreement—may be better known, and another objective of the mission will be to probe the geology of the Antarctic.

Another part of the extended operation of the *Glomar Challenger* that

has been greeted with enthusiasm by geophysicists is the proposal to drill more deeply than before into the rock underlying ocean sediments. Such an operation has been made possible by a technique which allows a previously drilled hole to be re-entered by a new drilling bit. This technique, which is accomplished by a sonar device, allows worn bits to be replaced, and was first successfully carried out in late 1970.

Gesture for Education

THE House of Representatives Committee on Science and Astronautics has again urged the National Science Foundation to spend more money on graduate student support, science education and institutional support. In what is becoming almost an annual ritual, the committee, on the recommendation of its subcommittee on science, research and development, two weeks ago reported out a bill which revamps the NSF's budget request, authorizing appropriations of \$680.0 million—\$27.8 million more than the foundation asked for—and taking money from other NSF activities to finance increases in its education support.

Last year, the committee transferred funds from the foundation's request for applied research to increase the budget for graduate student support and education, but this year the knife has been applied chiefly to the foundation's basic research programmes. In short, the committee has lopped \$4.3 million off the budget for scientific research project support and \$2.2 million off the request for national and special research programmes, but increased the appropriations request for institutional support by some \$11 million, for graduate support by \$10.8 million and for science education by some \$13 million. (The figures contained in the foundation's budget for 1973 include some money carried over from the present financial year.)

A staff member of the committee explained that the decision to take money away from the NSF's requests for basic science in no way represents disapproval of the programmes, but is simply a measure designed to increase the foundation's support of science education while keeping the overall budget increases to a minimum. The authorizations bill does not specify which programmes should be reduced, although it is recommending that \$2 million be chopped from the new technology incentives programme, and that the research and development assessment programme be reduced by \$200,000. One recommendation, which will be welcomed at least by astronomers, is that the committee has given its blessing to the Very Large Array system of antennas for radio astronomy.

As for the graduate student support programmes, the committee is recommending that the NSF's traineeship programme be reinstated on a rather limited basis. The Administration has been attempting to kill the traineeship programme for the past two years, chiefly because of the present unemployment situation among PhDs, but each year the Science and Astronautics Committee has increased the budget request. The chief result has in the past been that the Office of Management and Budget impounds the increases voted by Congress, and the foundation winds up that much the poorer. This year, however, the committee is suggesting that the traineeship programme be reinstated at least for those areas such as environmental sciences where manpower is likely to be in short supply.

Frustrated in the past in its endeavours to increase support for science education and graduate students, the committee this year has included in its authorizations bill some recommended changes in the National Science Foundation Act. Essentially these changes, if agreed by Congress, would raise education to the same level of importance as support of basic science in the NSF's activities. The changes, which were inserted in the authorizations bill by the full committee, would be a powerful expression of congressional support for science education and may possibly make the Office of Management and Budget, and the foundation itself, think twice before sanctioning any future cuts in the foundation's budget in this area.

The Science and Astronautics Committee is only one of four congressional committees concerned with the NSF's budget, and it essentially sets upper limits for appropriations. By far the most powerful of the four committees—the House Appropriations Committee—has in the past, however, followed the Science Committee's lead in urging greater expenditure on science education.

NEWS AND VIEWS

Teleseismic Source from a Ten Ton Charge

FOR more than half a century, earthquakes have provided the sources of seismic wave radiation which have been used to reveal the major internal structures of the Earth. Though great progress has been made, earthquakes are not ideal sources for this purpose because the mechanism of earthquakes is such that the seismic energy is radiated in rather complex patterns which make it difficult to separate amplitude information relevant to structure. Signal onset times have provided the primary data, but the location, origin time and depth of earthquakes must be inferred before they become useful for structural studies. Errors in these three parameters are reflected in subsequent analyses. Finally, earthquakes must be taken where they occur, and a seismicity map of the world shows that they occur in very limited and probably anomalous areas of the crust and upper mantle.

In 1957 Professor Keith Bullen suggested at a meeting of the International Union of Geodesy and Geophysics a way of getting round these problems by using nuclear explosions, the locations and origin times of which were known. At this same meeting, soon after Professor Bullen had drawn attention to the value of these explosions, the United States Atomic Energy Commission announced in advance the location and origin time of the world's first underground nuclear explosion.

The Atomic Energy Commission has since contributed the source parameters, including yield and geological details near the source, of many nuclear explosions which have occurred in the United States. The expected firing times of a few explosions were announced in advance to allow temporary stations to be deployed for specially designed experiments, and seismologists have made good use of these data, especially for investigating the deep structure of North America.

The sample of explosions is very small, however. France has helped by giving source data on some Sahara explosions, but the Soviet Union is strangely reluctant to reveal the precise location and times of its nuclear explosions. A selection of these explosions would be of inestimable value because they seem to have been used in many areas of the Soviet Union (in tests for engineering applications as well as weapons) and consequently they sample a much larger and structurally varied sector of the Earth than do earthquakes. To be sure, even without this information nuclear explosions provide evidence to show not only that earthquakes are indeed located along anomalous belts round the Earth but also that older, aseismic fold mountain structures may be underlain by similar structures. An example of this kind of evidence was reported by Anglin (*Nature*, **233**, 51; 1971), and because seismograms are susceptible to several interpretations, it becomes increasingly attractive to extend the application of explosion seismology. The release of more data would certainly help, but there must be a limit to the use of nuclear explosions, and in any case they are prohibited by treaty from the oceans.

It is just this environment, however, that the ideas of Willmore and his colleagues at the Institute of Geological Sciences Global Seismology Unit in Edinburgh can be put into practice. On page 305 of

this issue of *Nature* Jacob and Willmore report the results of firing 10 tons of chemical explosive in the North Sea. Though the experiment was deliberately not announced, signals were reported from standard stations as distant as Brasilia (83°) and from Brisbane at the PKP caustic, so a close scrutiny of the standard station microfilms is expected to provide a valuable set of world-wide data.

The observation that water is a more efficient coupling agent than rock had, of course, been made many years ago and much use has been made in Britain of the Royal Navy's obsolete depth charges for seismological research. On a grander scale, Liberty ships filled with a kiloton or two of time lapsed explosives, and fired in the Atlantic and the Pacific Oceans, have provided world-wide seismic wave coverage in support of the United States programmes. The novelty of Jacob and Willmore's experiment is the care taken to optimize the charge weight and depth of firing so as to radiate seismic energy in the pass band of the short period instruments at the standard stations. In effect they have created a teleseismic source from 10 tons of chemical explosive. At the mid-point of the short period band the charge which meets the conditions is 18 tons, but the Naval Construction Research Establishment preferred 10 tons (peaking at 1.9 Hz) for convenience in handling. The charge was suspended from a floating buoy and fired electrically at a depth of 200 m.

The detection of the admittedly small signal was assisted by the fact that it had the predicted frequency of 1.9 Hz, and though only a few distant standard stations responded to a questionnaire (the magnitude averages m_b 4.38 at five stations between 56° and 83°) it is likely that more data will accumulate after a closer reading of the records. In any case, the Yellowknife array (51°) clearly shows the advantages to be gained by deploying temporary stations having magnetic tape recorders.

Jacob and Willmore refer to the possible use of the method for mapping the laterally varying properties of the upper mantle, and for this purpose the development could not have been better timed, for the efforts of most experimental seismologists are now drawn to this area. It would be interesting to see what the data from this experiment alone reveal about the attenuation of high frequency teleseismic P waves in the upper mantle. The quoted magnitudes of the distant stations suggest that the signal will be best observed on geological "shields"; the magnitudes of the shield stations average m_b 4.75. Explosions on land give a similar result, and the amplitudes are maximum if the source is also on a shield area.

If the problems of handling 10 ton charges on the deep oceans can be mastered, the present constraints on representative sampling of upper mantle structures may well be relaxed. Both sea to continent and sea to sea paths can be included in the experimental design and ocean bottom seismographs, in which the Institute of Geological Sciences has made an investment, should have a useful role here. If the development of the method fulfills the early promise, and integrated teams can be organized, it is possible that the geophysicist's views of the upper mantle, which have been shaped by earthquakes, may have to be modified.—From a Correspondent.

Neuronal Specificity

IN the development of a vertebrate, nerves carrying information from peripheral sense organs make highly specific connexions with their appropriate receiving areas in the central nervous system. One of the most interesting problems of developmental biology today is to determine how such specificity exists and how it is determined. In the visual system of an amphibian, for example, are ganglion cells in the retina labelled in some way so that their axons, on growing back to the optic tectum, can only make specific connexions within a set of one or more similarly labelled central neurones? Superficially this seems an easy question to answer, but, as two recent books by Gaze (*The Making of Neural Connexions*, Academic Press, 1970) and Jacobson (*Developmental Neurobiology*, Holt, Reinhart and Winston, New York, 1970) show only too clearly, the problem is still largely unsolved after at least fifty years of experimental attack.

Theories of neuronal specification can be grouped into two classes—those in which central and peripheral neurones have matching labels and can only make quite specific connexions within a small region, and those in which there is a global map within which axonal terminals find their right terminations by “reading” a coordinate system. The problem is also complicated by the possible lability of connexions, both during normal development and as a result of subsequent learning during which environmental stimuli might play a part in determining the final specificity of both connexion and function. These problems are well illustrated by two recent reports: Gaze, Chung and Keating (*Nature New Biology*, **236**, 133; 1972) and Baker (*Nature*, **236**, 235; 1972).

Gaze *et al.* recorded from the optic tecta of *Xenopus* tadpoles of various developmental ages up to metamorphosis. The experiments were carried out on the animals underwater, which is important because much optical distortion is thereby avoided. Each set of recordings involved mapping the projection from one retina to the contralateral tectum by way of single-unit responses evoked with retinal stimulation by light spots or by black disks on a light background. In very young tadpoles (between stages 44 and 48) the projection is very abnormal. Receptive fields are extremely large, subtending up to 100 degrees, and nasal and temporal poles of the visual field may project to a single tectal locus. After stage 48 receptive fields gradually become smaller, and the normal ordering of the retino-tectal projection emerges, that is, the nasal part of the visual field, seen by temporal retina, projects to the rostral part of the tectum while the superior field projects medially.

There are other changes in the projection during development. First, in early tadpoles sometimes only the front half of the tectum receives visual input, whereas the whole of the tectum does in late tadpoles. There is some correlation with the degree of differentiation observed histologically in the tectum, the front being more advanced. Second, the projection in the adult is approximately linear in that the stimulus subtending a given retinal angle stimulates a constant hyphen-sized tectal region, whereas in early tadpoles only a small part of the temporal visual field sends a highly magnified projection to the back of the tectum.

The mapping of retina on tectum in *Xenopus* therefore

changes during normal life in such a way that particular retinal ganglion cells connect with the continuously changing set of tectal neurones. It therefore seems unlikely that a precise chemical labelling mechanism is involved, for the labels would have to change with changes in the connexion pattern. This obviously makes it harder to understand the mechanism whereby retinal and tectal growth are matched.

The retina grows by angular accretion, and the tectum grows by expansion from front to back and from side to middle. Although it is possible to imagine how two such topologies could map into each other and preserve connectivity, the results Gaze *et al.* report show that this cannot be the case. The coordinate system which specifies position within both retina and tectum must therefore also change during development. It is important to realize this when considering results of experiments in adult *Xenopus* with cut optic nerves the fibres of which regenerate to their correct terminations in the tectum. Here one map projects directly to another, without correlated growth and corresponding change in each coordinate system. One caveat that should be mentioned is a problem of accuracy in making retino-tectal maps in very small tadpoles. It is clearly important to be able to show qualitative changes in the projection during development, as Gaze *et al.* report.

Baker has been studying the derangement of a behavioural response in *Bufo* and in *Rana pipiens*. The response is the adult wiping reflex in which a noxious stimulus causes accurately directed wiping of the skin by the appropriate limb. If, for example, the skin from a tadpole's back is exchanged with skin from its belly, then in the adult, under certain conditions, the wiping reflex will be misdirected. The graft must be done early in the tadpole's life (before stage 15). Baker has extended this sort of experiment by removing back skin and placing it on the belly, but not making the reciprocal graft of the removed belly skin to the back. The epithelium of the back regenerates, so that the adult animal has back-type skin both on its belly and its back and a correspondingly reduced amount of belly type skin. Experiments on fourteen *Bufo* tadpoles and three *Rana* tadpoles showed that stimulation of the regenerated back skin elicited normal reflex behaviour, and stimulation of the misplaced back skin elicited a misdirected reflex.

Baker considers that this result militates against both specific regrowth of cut axons to their original innervation sites and of similar specific regrowth after axonal degeneration. It is still possible, however, that there is specific regrowth and that the same set of axons regenerates to both the original and the new back skin, either by redistribution, or by regrowth with extra branching. He feels also that the third possibility he had considered—that of growth of axons from “spare” neurones which had not taken part in the original skin innovation—is hard to disprove. What seems to be well established is that skin type determines the nature of the growth response, either by determining which axons reinnervate the skin, or by determining more central connexion patterns. In other words, the problem is still open.

Both these articles are interesting but both inevitably leave major problems unsolved, although it now seems clear that Sperry's original concept of strict neuronal specificity cannot be retained.—A. D. J. R.

Depth of Origin of Gravity Anomalies

ACCURATE determinations of the orbits of artificial satellites have led to a much more detailed knowledge of horizontal variations of the Earth's gravitational field than could be obtained from ground-based gravity measurements alone. These variations are important to geophysicists because they reflect the density distribution in the Earth's solid mantle and crust and the solid inner core. Unfortunately, there is no unique mathematical solution to the problem of determining the density distribution from the gravitational field alone and progress cannot be made without introducing additional geophysical information or hypothesis.

The discovery of a certain association of the Earth's magnetic and gravitational fields (R. Hide and S. R. C. Malin, *Nature*, **225**, 605; 1970) is compelling evidence that density variations in the deep mantle and at the interface where the mantle meets the liquid iron core contribute significantly to the large-scale features of the gravitational field, but smaller-scale features, with horizontal scales less than about 1,500 km, must arise largely in the crust and upper reaches of the mantle. This expectation accords fairly well with the demonstration that the degree of correlation between the gravitational field and the Earth's surface features is very much better for small and medium-scale observations than it is for large-scale variations (A. H. Cook, *Nature*, **198**, 1186; 1963 and W. M. Kaula, *Science*, **169**, 982; 1970).

Some workers, however, choose to neglect the magnetic evidence and place the origin of all the gravity anomalies in the upper mantle. Thus Bott (*Earth Planet. Sci. Lett.*, **11**, 28; 1971) prefers a depth of 410 km, where seismological evidence indicates a small density discontinuity, and Higbie and Stacey (*Phys. Earth Planet. Interiors*, **4**, 145; 1971) prefer somewhere between 250 and 900 km, where mechanical stresses would be roughly independent of horizontal scale. In an important article in a recent issue of *Nature Physical Science* (**236**, 23; 1972), R. R. Allan, by developing a method due originally to Guier and Newton (*J. Geophys. Res.*, **70**, 4613; 1965) and applying it to the Gaposchkin and

Lambeck model of the gravitational field (*J. Geophys. Res.*, **76**, 4855; 1971), has made a useful step towards the resolution of the confused situation regarding the depth of origin of gravity anomalies.

Allan's method rests on the assumptions that gravity anomalies arise from many independent density variations and the correlation between the density variations at different points falls to zero as the distance between the points increases. According to Allan, separate sources are required for the small-scale and large-scale anomalies, with the

former arising at depths between 230 and 350 km and large-scale anomalies very much deeper down.

The gravitational field of the Earth is now known with such great accuracy and high resolution that the application of all relevant geophysical and geochemical evidence—not just gravitational and seismological—to the determination of the density distribution within the solid part of the Earth must surely constitute one of the next important steps towards a satisfactory model of its structure and evolution.—From a Correspondent.

GLOBAL TECTONICS

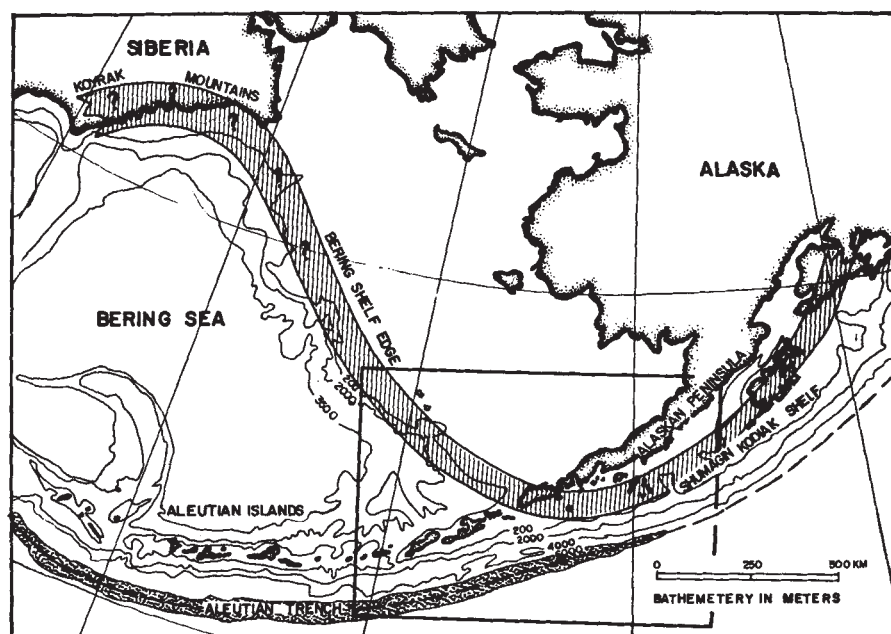
Old North Pacific Trench

from our Geomagnetism Correspondent

THE spreading ocean floor in the northern Pacific is downwelling at present along the Aleutian trench which marks the northern boundary of the Pacific crustal plate. According to Scholl *et al.* (*Bull. Geol. Soc. Amer.*, **81**, 3583; 1970) the Aleutian trench and its associated island arc system were probably initiated as recently as the late Cretaceous or early Tertiary; and presumably the present geometrical configuration of spreading in the Aleutian region dates from that time. But what was the pattern of spreading and crustal subduction before the formation of the Aleutian trench? Moore (*Science*, **175**,

1103; 1972) has now adduced evidence which suggests that during the Cretaceous the northern Pacific trench lay north of its present position. In the east, the difference between the present and Cretaceous positions is but a few hundred kilometres; but in the west the Cretaceous trench lay north of what is now the Bering Sea (see figure).

Moore's evidence comes largely from a study of Cretaceous turbidites and primary sedimentary structures along the south-west coast of Alaska adjacent to the Alaskan Peninsula. The Shamugin-Kodiak shelf to the south-east of the Alaskan Peninsula is known to



Moore's suggested location of the north Pacific trench (vertically hatched region) during the Cretaceous (from *Science*, **175**, 1104; 1972).

comprise a deepwater igneous and sedimentary sequence which contains basic volcanics, ultramafic rocks, bedded cherts and a series of folded and interbedded sandstones and mudstones. The ages involved here range from Triassic to Eocene, which suggests a correlation between the rocks of the Shumagin-Kodiak shelf and the Mesozoic sedimentary and volcanic sequence in the Koryak Mountain region of eastern Siberia. Indeed, Burk (*Mem. Geol. Soc. Amer.*, **99**; 1965) and, more recently, Scholl *et al.* (in the press) have gone so far as to suggest that the two sequences were joined through the Bering shelf edge, thus forming a continuous province.

What Moore has tried to show is that the sedimentary components of this province were deposited in a trench and thus that the province itself is the site of the pre-Aleutian trench subduction system. Most of the exposed rocks along the Shumagin-Kodiak shelf are Cretaceous deepwater sediments which were apparently deposited by turbidity currents. The sandstones comprise almost 50 per cent rock fragments, most of which are andesitic volcanic detritus which Moore interprets as being derived from a primary source. Because modern trenches are almost always associated with composite volcanoes on their landward sides, the existence of abundant volcanic detritus in the turbidites is thus at least consistent with trench activity though perhaps not proof of it.

More important are the basal current markings from which may be deduced the directions of the flow in the Cretaceous turbidites. In the Shumagin-Kodiak shelf turbidites investigated by Moore the palaeocurrent flow directions are parallel to the present continental shelf—that is, parallel to the trend of the province which Moore is trying to establish as the ancient trench; for example, in the Shumagin Islands, which lie away from the curve towards the Bering shelf edge on that part of the Shumagin-Kodiak shelf which trends south-west, the Cretaceous flow direction was $212^\circ \pm 42^\circ$ ETN (standard deviation).

Such directions are inconsistent with the transport by turbidity currents across either continental rises or deep sea fans. They are consistent with a flow along a marginal ocean basin coinciding with the longitudinal province under investigation; but such a basin would have to have been bordered on its seaward side by a basement which was both uplifted and older than the sediments deposited in it. There are, in fact, no rocks older than the Cretaceous sediment on the seaward side of the Shumagin-Kodiak or Bering shelf edges. The only other possibility seems to be that of a deep ocean trench which

confined the turbidity currents to flow longitudinally.

Accordingly, Moore suggests that the Koryak-Bering-Shumagin-Kodiak zone is the site of a trench along which the spreading north Pacific was consumed prior to the initiation of the Aleutian trench. In other words, somewhere around 100 million years ago the crustal boundary of the north Pacific changed, leading to the formation of the Bering Sea.

TISSUE CULTURE

Differentiation *in vitro*

from our Cell Biology Correspondent

SINCE its introduction the technique of fusing pairs of somatic cells to yield hybrid cells with one nucleus containing two genomes of different origin has been widely exploited by investigators hoping to elucidate the molecular mechanisms of cellular differentiation. The typical experiment involves the fusion of a differentiated cell, which in culture continues to express some specific and readily assayable functions, with either a differently differentiated cell or a fibroblast. The resultant hybrid cells are then assayed for the continued expression of the specific functions of the parental cells.

Enough experiments of this sort have now been carried out for one to conclude with confidence that although specific differentiated characteristics tend to be extinguished in the hybrids—for example, hybrids of pigmented melanoma cells and unpigmented fibroblasts are unpigmented—it is by no means universally the case. Some differentiated functions have been found to survive in particular hybrid cells and, as the experiments of Peterson and Weiss (*Proc. US Nat. Acad. Sci.*, **69**, 571; 1972) indicate, occasionally it is even possible by hybridization to induce the expression of a differentiated function which in the parental cell is not expressed.

Weiss and her colleagues reported last year (*ibid.*, **68**, 127; 1971) the production of a series of clones of hybrid cells obtained by fusing rat hepatoma cells with murine 3T3 fibroblasts in a medium which selects for hybrids. Two of the three differentiated functions of the hepatoma cells are extinguished on hybridization, for the hybrid cells fail to produce type B fructose 1,6-diphosphate aldolase or, in response to steroid hormones, tyrosine aminotransferase. Immunodiffusion tests reveal that cells of some of the hybrid clones also fail to produce rat serum albumin but cells of other clones either produce rat albumin or rat and mouse albumins or

Hormone Dependent Cell Line

INVESTIGATIONS at the molecular level of how hormones act on target cells have been greatly facilitated by the selection of stable cell lines which not only grow in tissue culture but also exhibit some functional response to particular hormones when they are added to the medium. Indeed, the analysis of the induction of changes in the pattern of gene expression by hormones can, in at least some cases, now be tackled directly by the techniques and ideologies that have arisen from investigations of the regulation of gene expression in *Escherichia coli*. But apart from those cells which respond by making this or that enzyme when exposed to hormones there are cells (gonadal cells are a classic example) which *in vivo* proliferate only when some particular hormone is present.

Attempts in the past to select stable cell lines the growth of which in culture depends on hormones as it does *in vivo* have been infrequent and for the most part unrewarding. As the report by Clark, Jones, Gospodarowicz and Sato in next Wednesday's *Nature New Biology* (April 12) proves, however, the problems of isolating a gonadal cell line which shows a very marked stimulation of multiplication in response to gonadotrophic hormones are not insuperable.

Clark and his colleagues implanted into the spleens of castrated female rats luteal phase rat ovaries; in these castrates serum concentrations of gonadotrophins increase some four to fifteenfold and the growth of gonadotrophin-responsive cells in the spleen implants should be selectively enhanced.

After about six months the nodules of ovarian cells in the spleens were dissected out and dispersed and then plated in serum containing tissue culture medium supplemented with bovine luteinizing hormone. One plate yielded epithelial cells, rather than the ubiquitous fibroblasts, which after 4 months in culture now double every 20 hours in the presence of the bovine luteinizing hormone.

Dexamethasone, insulin and porcine follicle stimulating hormone all act synergistically with bovine luteinizing hormone in promoting the proliferation of these rat ovarian cells, but several other hormones fail to elicit any growth response. Attempts to select even more hormone responsive sublines using a fluorodeoxyuridine killing technique have so far failed but even with the currently available cells it should now be possible to study *in vitro* the biochemistry of the stimulation of ovarian cells by hormones.

mouse but not rat albumin; these last clones have presumably lost the chromosome carrying the structural gene(s) for rat albumin.

Those hybrid cells which make both rat and mouse albumins were obtained from crosses of 3T3 cells and hypertetraploid rather than hyperdiploid rat hepatoma cells and examination of the hybrid cell karyotypes revealed that the induction of the production of mouse albumin is correlated with a karyotype containing high proportions of rat chromosomes. Indeed, it seems most likely that the continued production of rat albumin as well as the induction of mouse albumin synthesis depends on both the dosage and the balance of both structural and regulatory genes. These findings also indicate that in rat hepatoma cells the production of albumin is not coordinated with the synthesis of the two assayed enzymes and that the mouse genome can respond to signals which activate the rat genome to make albumin.

In the same issue of the *Proceedings* (*ibid.*, 69, 566; 1972) Seecof, Teplitz, Gerson, Ikeda and Donady report a method for establishing cultures of *Drosophila melanogaster* neuroblasts and myoblasts and observing *in vitro* not only the differentiation of these cells into myocytes and neurones but also the differentiation of active neuromuscular junctions. These junctions, which under the electron microscope have a fine structure entirely typical of the insect neuromuscular junction, form where axons come into contact with myocytes; the junctions respond to electrical stimuli by innervating the myocytes which seem to fall into two physiological classes: those which respond to long trains of pulses and those which respond to much shorter pulse trains.

As Seecof and his colleagues note, although the differentiation of vertebrate neuromuscular junctions has been observed in tissue culture systems, the *Drosophila* system which they have perfected is unique in that all stages of differentiation from myoblasts and neuroblasts to myocytes and neurones connected by neuromuscular junctions can be observed *in vitro*. And once appropriate mutants become available the potentialities of this system for the genetic analysis of neuromuscular differentiation seem great indeed.

PRECAMBRIAN

Crustal History

from a Correspondent

AT the Royal Society on March 22 and 23 a discussion meeting on the evolution of the Precambrian crust brought together workers on the Precambrian from Canada, Greenland, Southern and

Central Africa, Scandinavia and the United Kingdom. One purpose of the meeting was to draw on collective experience in these widely separated areas and to see to what extent this experience could be used to obtain a picture of the behaviour of the Earth in the early Precambrian. In opening the discussion Professor R. M. Shackleton (University of Leeds) drew attention to several questions which can now be asked. When and how quickly did the granitic crust form and how extensive was it; were the early greenstone belts floored by granitic crust; how extensive were these belts?

The opening session was concerned with the earliest gneisses, and some of the questions posed by Professor Shackleton came up repeatedly. The recognition that granitic rocks 3.8×10^9 years old are present in West Greenland provides critical evidence that the granitic crust was in being at that time. Dr S. Moorbath (University of Oxford), in whose laboratory the age of these rocks was established, demonstrated among other matters the usefulness of studies of the isotopic composition of these early Precambrian masses in determining early crustal history. Dr V. R. McGregor (Sukkertoppen, West Greenland) described the

detailed geology of the area in which he had found these very old rocks and emphasized the granitic nature of the material 3.8×10^9 years old. Considerable attention was paid to problems of granulite facies metamorphism. Can it occur only when continental crust has reached a certain thickness, or when the original heat flow has dropped below some critical level?

On the second day Professor K. S. Heier (University of Oslo) drew attention to the marked discrepancy between the abundance of uranium, thorium and potassium indicated by geochemical sampling near the Earth's surface and the lower values of these heat producing elements deduced from heat flow measurements. It follows that the abundance of these elements near the surface must be considerably greater than lower in the crust; this would be compatible with a model which envisages granitic rocks high in the crust and granulites at depth, as Windley and Bridgwater have suggested. Dr J. V. Watson (Imperial College, London) showed how the Scottish Lewisian seems to conform to such an arrangement and suggested that the survival of granulite facies rocks at depth is a feature of more than one metamorphic belt in which

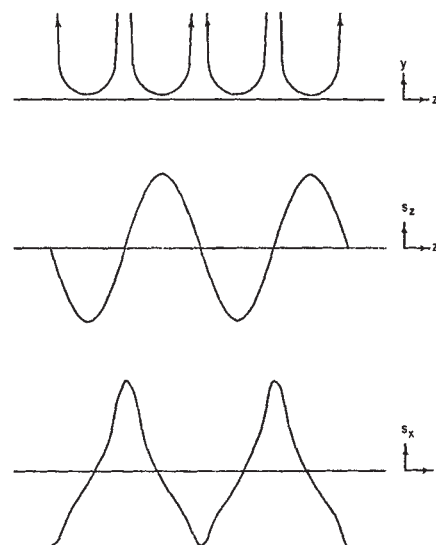
Wall Eddies and Drag Reduction

It is now well known that frictional drag in liquids can be reduced by the addition of small amounts of certain polymers. In the presence of only a few parts per million of some polymers, drag reduction (as measured by the drop in pressure gradient) occurs in flow through a circular pipe, for example, provided that the flow is turbulent, and that the shear stress at the pipe wall is greater than a critical value. It turns out that the drag reduction is increased as the pipe diameter is reduced—for a given concentration and Reynolds number.

Although the phenomenon is well documented, its explanation is unclear. One of the recently canvassed ideas is that increased drag reduction is associated with an increase in the separation ("wavelength") of those eddies that are close to the wall and oriented in the direction of the mean flow.

In next Monday's *Nature Physical Science* (April 10), Eckelman, Fortuna and Hanratty provide striking evidence that this increase in the eddy wavelength does, in fact, occur. The figure here shows the ideal eddy pattern very close to a wall at which drag occurs; the mean direction of flow is in the x direction, perpendicular to the plane of the paper, and s_x and s_z are two of the components of the fluctuating velocity gradient.

The wavelengths described by Eckelman *et al.* are those of the bottom dis-



tribution in the figure as measured previously by Fortuna and Hanratty with ten small mass transfer probes fitted flush into the wall of a pipe, but reduced to a dimensionless form. In a real situation, of course, the s_x pattern is irregular, but, after making a few assumptions, Eckelman *et al.* come to the conclusion that the dimensionless wavelengths, relative to that in a Newtonian fluid, are 1.6 for a drag reduction of 27 per cent, 3.2 for a reduction of 49 per cent, and 5.0 for a 65 per cent drop.

retrograde metamorphism had affected material higher in the crust.

Although it is now known that there was a granitic crust 3.8×10^9 years ago, it is still uncertain as to how extensive it was. Dr C. R. Anhaeusser (University of Witwatersrand) discussed the nature of the crust on which the volcanic and sedimentary rocks of Barberton in South Africa were deposited at least 3.4×10^9 years ago and concluded that it might have been oceanic.

Dr J. F. Wilson (University of Rhodesia) described areas where greenstone belts in Rhodesia are underlain by granitic rocks, but it is possible that these granitic rocks are younger than the Barberton succession, so that it is still uncertain whether southern Africa contains granitic rocks formed before the earliest greenstone belts. These are, however, interesting parallels between the oldest rocks described by Dr Wilson and those from West Greenland known to have formed 3.9×10^9 years ago. In each instance there is a record of repeated intrusion of granitic rock. Several speakers suggested that the make-up of the earliest crust indicated an evolution not through volcanic or sedimentary activity so much as by intrusion, deformation and recrystallization of acid plutonic rock.

Dr C. J. Talbot (University of Dundee) reviewed the possible models which might explain the occurrence of greenstone belts and the adjoining granitic rocks and suggested a mechanism by which small masses of continental crust could be built up by convection. The question of the amount of horizontal movement was brought out in a discussion of the history of Proterozoic fold belts.

Dr R. Mason (R. F. Loxton, Hunting and Associates, Johannesburg) showed striking illustrations of the Limpopo Belt formed probably between 2.8 and 2×10^9 years ago. This belt is remarkable both for the granulite facies metamorphism and for the belts of straightening in which intensively deformed rocks form linear structures within the Limpopo Belt. Dr M. Coward (University of Leeds) suggested that the Limpopo Belt was marked by a transcurrent movement of some 200 km between the blocks on either side.

In another context Dr Watson discussed such large linear structures when contrasting the behaviour of granulite facies and lower grade rocks during deformation. She pointed to evidence in Scotland which indicates that granulite facies rocks survived at depth when retrograde metamorphism had occurred higher in the crust and suggested that Wegman's *Unterbau*, the zone of intense metamorphism, might have a lower boundary. This concept was consistent both with the Bridgwater-Windley model of the crust in the early

Precambrian, with a granitic upper part and a granulite-rich lower part. There now seems to be considerable evidence, from studies of heat flow, the distribution of granitic rocks and from structural investigations, that from at least 3.2×10^9 years onwards granulites overlain by granite-rich zone formed an important element in continental crust.

In the final session further examples of Proterozoic deformation were described from Greenland, where basic and ultramafic dyke swarms have provided Drs D. Bridgwater and A. Escher (Geological Survey of Greenland) and Dr J. Watterson (University of Liverpool) with a means of measuring deformation within the Nagssugtoqidian fold belt. This belt contrasts with the Ketilidian of South Greenland formed at roughly the same period, and Dr C. Pulvertaft (Geological Survey of Greenland) joined with these three workers in suggesting how these contrasts reflected long continued differences in crustal behaviour.

Dr P. Hoffman (University of California, Santa Barbara) discussed the Coronation geosyncline in the north-western Canadian Shield which, though formed more than 1.75×10^9 years ago, he found to be distinguishable only in detail from the Cordilleran or Appalachian geosynclines. This contribution brought the discussion up to the point in time—some 2×10^9 years ago—when Precambrian geological features took on forms familiar through the remainder of geological history.

CONSERVATION

Odendaal Report

from a South African Correspondent

SINCE the publication in 1964 of the *Report of the Commission of Enquiry into South West African Affairs, 1962-63* (the Odendaal Commission), conservationists have bemoaned the implication of this report for the wildlife of northern South West Africa, and particularly the Etosha Game Park and the Kaokoveld. Unfortunately there has been little agreement as to either the dangers involved or the possible remedies.

The area concerned is that of the so-called Game Reserve No. 2 which consists of three parts—the Etosha Game Park, a dual-purpose reserve for Africans and wildlife in the northern Kaokoveld, and the Kaokoveld Nature Reserve south of this (R. C. Bigalke, *Afr. Wild Life*, 18, 181; 1964). In terms of the Odendaal Report the dual-purpose reserve is to be deproclaimed, roughly 1,600 square miles are to be excised from the northern and western parts of Etosha Game Park and most of the Kaokoveld Nature Reserve is to be incorporated into a new homeland, Damaraland. A 20-mile strip of the Namib desert, extending from the Kunene river to near Swakopmund, is instead to become a Skeleton Coast reserve. Writing at the request of the Wildlife Protection and Conservation

Patterns of Actin Activity

X-RAY diffraction techniques can be used to study the structure of muscle, not only in the living resting state, but also in the actively contracting state and in the rigor state. A great deal of interest obviously centres on the actively contracting state and the structural changes that are associated with it. When vertebrate striated muscle is activated there are considerable changes in the X-ray pattern arising from the myosin filament structures, which are attributed to asynchronous movement of the cross-bridges. There are, however, no immediately obvious changes in the actin pattern.

Recently, there have been more detailed X-ray studies of the contracting and rigor states, and evidence has been accumulating which suggests that there may also be a structural change in the actin filaments associated with activity. In X-ray patterns from contracting vertebrate striated muscle Huxley (*Eighth Intern. Cong. Biochem.*, Interlaken, 1970) observed a diffuse reflexion which was not seen in patterns from the resting muscle, and which he identified as the second layer line of the actin helix. Now, in an article to

be published in next Wednesday's *Nature New Biology* (April 12), Vibert, Hazelgrove, Lowy and Poulsen from Aarhus report that in both vertebrate and invertebrate smooth muscles there are changes in the intensities of certain of the actin layer lines when the muscles are activated; in the invertebrate muscle (anterior byssus retractor of *Mytilus*) the intensity of the second layer line is preferentially increased. The indications are, therefore, that there may be similar changes in the actin filaments of all different kinds of muscles on activation.

The Aarhus group have also observed that in X-ray patterns from vertebrate striated muscle in rigor there is an increase in the intensity of the second actin layer line, and that this occurs irrespective of the degree of overlap of the actin and myosin filaments.

Vibert *et al.* conclude that the actin filaments can exist in two different configurations, one corresponding to a resting state, and the other to an activated state, and that the change from one to the other is not dependent on the presence of myosin.

Society, Bigalke suggested as an alternative that the western end of Etosha, instead of being excised, be retained and slightly extended to conserve rhino, mountain zebra and impala; that a north-eastern corridor, extending from eastern Etosha to the Okavango, be left unoccupied to serve as a migration route for eland, wildebeest and zebra; and that a buffer zone be created north of Etosha to include the wintering area of wildebeest, zebra and springbok herds which summer in the park. He further suggested that the (probably inviable) Skeleton Coast reserve be extended 10 miles inland and that the Okavango and Caprivi be set aside for species such as lechwe, puku and sitatunga, which do not occur elsewhere in South West Africa.

In a subsequent report K. L. Tinley, also writing on behalf of the Wildlife Society (*Afr. Wild Life*, 25 (1) Suppl., 1; 1971), presents an entirely different viewpoint. Ignoring Bigalke's proposals, and in fact not even referring to his report, he proposes instead that a Kaokoveld National Park be established and joined by an enclave to the Etosha Park (in more or less the fashion that the Kaokoveld Nature Reserve is joined to the park, but covering a wider area); that a large Kunene National Park be established south of the Kunene river; and that a narrow triangle of land corresponding to Bigalke's "buffer zone" be added to the north-east of the Etosha Park. No mention is made of the migration route to the Okavango, or of Bigalke's proposed reserves in Okavango and the Caprivi, and where Bigalke suggested retaining or increasing the western end of Etosha, Tinley is prepared to sacrifice an even larger part than is called for by the Odendaal Report.

The Wildlife Society, in endorsing Tinley's views as it had previously done for Bigalke's, gives no indication as to why it has apparently undergone a major change of heart in the seven years separating the two reports.

The most recent report, an independent one by an agriculturist, G. L. Owen-Smith (*S. Afr. J. Sci.*, 68, 29; 1972), deplores the "considerable contradiction and confusion as to the actual issues involved" on the part of conservationists. Having said this he proposes yet a third solution, which involves accepting the Odendaal Commission boundaries for the Etosha Park, but considerably widening the Skeleton Coast Park, to more or less the distance inland that Tinley suggested for his Kaokoveld National Park, and extending all the way from the Kunene to the Ugab river. He does not suggest linking this reserve to the Etosha, because (*pace* Tinley) "no evidence was found of any large scale migrations between the Etosha Game Park and the western

Kaokoveld"; neither does he consider either adding any land to the park or safeguarding migration routes eastward to the Okavango.

It is unlikely that any sort of representation by conservationists will influence the implementation or otherwise of the Odendaal Report. Assuming that they could, however, these conflicting reports cannot help their cause.

PLANT NUTRITION

Need for Nickel

from a Correspondent

It is only during the past twelve years that serious attention has been given to the use of certain plant species as indicators in geochemical prospecting. The potential value of such plants lies in their capacity to accumulate certain mineral elements to concentrations far greater than those found in the parent rock and soils in which they grow. This in turn facilitates the detection and measurement of elements present in the rocks at very low concentration. The list of plants which behave in this way has now grown extensively, most work in this field having centred in Australia, Africa, the Soviet Union and the United States. A recent addition to the list is discussed in the current issue of *Planta* (103, 91; 1972), where B. C. Severne and R. R. Brooks, of Massey University, New Zealand, describe *Hyban-*

thus floribundus, a plant which is capable of accumulating nickel to concentrations hitherto unknown in the plant kingdom.

Hybanthus floribundus is a shrubby member of the Violaceae which is widely distributed in western and southern Australia. Chemical analyses of populations of this species from the Eastern Goldfields area of Western Australia were undertaken by Severne and Brooks using atomic absorption spectrophotometry. In some of the samples they found that the nickel content of the ashed leaves was as high as 23 per cent, a concentration considerably higher than that of the local soils (which average 670 p.p.m. nickel).

The usefulness of a plant species in geochemical prospecting, however, depends on a linear relationship between the concentration of the element in the soil and its relative accumulation in the plant (expressed as plant concentration/soil concentration). Two years ago Timperley, Brooks and Peterson (*J. Appl. Ecol.*, 7, 429; 1970) demonstrated that such linear relationships are found only in the case of elements which are not essential for plant nutrition. They found that the relationship between relative accumulation and element concentration for essential elements was hyperbolic; high accumulation occurs at low concentration and little or no accumulation occurs at high concentration. The curve flattened at the point

Messenger and Heterogeneous RNA's Kinship

THE so-called giant heterogeneous RNA which is rapidly synthesized and also rapidly degraded within the nucleus of eukaryotic cells has long been the centre of controversy. The point at issue is the relationship, if any, between this nuclear RNA and cytoplasmic messenger RNA. One theory is that the giant nuclear RNA contains precursors to messenger RNA based on the evidence from hybridization experiments and studies of the kinetics of labelling of these RNAs. Other workers, by contrast, argue that there is no relationship between heterogeneous giant nuclear RNA and cytoplasmic messengers. This second view is becoming less fashionable, not least because in the case of SV40 transformed fibroblasts a precursor-product relationship between giant nuclear and messenger RNA has been established beyond reasonable doubt. But what is needed is a general method for investigating the relationship between these two classes of RNA, and that is what Melli and Pemberton describe in next Wednesday's *Nature New Biology* (April 12).

Melli and Pemberton have used *Micrococcus lysodeikticus* RNA polymerase to synthesize an RNA comple-

mentary to the 10S RNA fraction that can be isolated from duck reticulocytes and is known to contain globin messenger RNA molecules. This antimessenger can be labelled to high specific activities, and fingerprint analyses of a ribonuclease T1 digest indicate that most if not all of the globin messenger sequence is transcribed by the *Micrococcus* enzyme into antimessenger. The hot antimessenger can then be used as a probe for complementary sequences in any RNA sample, and by comparing the rates of hybridization between antimessenger and 10S messenger RNA or heterogeneous nuclear RNA from reticulocytes it is possible not only to detect messenger sequences in the heterogeneous nuclear RNA but also to estimate their concentration.

In short, as soon as messenger RNAs become available in adequate quantities they can be used as a template for the synthesis of very hot antimessengers by the *Micrococcus* enzyme; the hot antimessenger can then be used as a probe for precursors to those messenger RNAs. The only snag is that at present the number of even partially purified messenger RNA preparations available is very limited.

where the plant's requirements were satisfied, and only in this linear part of the curve was the plant of value for prospecting.

When they examined nickel accumulation in *Hybanthus*, Severne and Brooks found that the relationship between accumulation and nickel concentration was hyperbolic, suggesting that the plant has a physiological requirement for nickel. Such a requirement is difficult to explain because nickel is normally regarded as a non-essential element in plant nutrition. If a need for nickel exists in this species, however, it would seem to be restricted to the Eastern Goldfields area of Australia because populations of *Hybanthus* from elsewhere in the country do not possess this capacity for nickel accumulation. It may be that the Eastern Goldfields populations of *Hybanthus* have developed this physiological adaptation as a response to a localized selection pressure. Thus although *Hybanthus* will be of limited value for the biogeochemical prospector, it poses some interesting problems to the plant physiologist and ecologist.

SERRANID FISHES

American Groupers

from our Marine Vertebrate Correspondent

It is a paradox of zoology that in general it is the largest animals which are the least known or studied, and this is particularly true of systematic studies which traditionally were laboratory based. One group of large fishes which illustrates this paradox are the groupers, or sea basses of the family Serranidae. The family forms an important component of the tropical and subtropical marine shore fauna; many of its members are large and in places are important food and game fishes. But there has remained a considerable area of doubt concerning the nomenclature and relationships of many of these fishes principally because their size has precluded the preservation of adequate material for study. The groupers (defined as the members of the genus *Epinephelus*, or its numerous synonyms) of the coasts of America have recently been critically reviewed by C. Lavett Smith (*Bull. Amer. Mus. Nat. Hist.*, **146** (2), 67; 1971) with considerable clarification of nomenclature and of their relationships.

Smith admits that the world-wide classification of the serranid fishes remains unsatisfactory, but in view of the size of the family (some 300 species are recognized) and because the physical size of many of the species makes museum collections unrepresentative, study of the forms around the American continent gives a clearer view of the

general problem. The groupers (some species are known as jewfishes and hinds) are well represented in American warmer seas, and Smith recognizes some thirty-five species. The Western Atlantic and Eastern Pacific species are intimately related, and several species occur in both oceans, for example, the jewfish, *Epinephelus (Promicrops) itajara*, which reaches a weight of more than 700 pounds (317 kg) and is the largest of all American groupers. In spite of its size this fish is found in the shallow shore zone, where it feeds mostly on crustaceans.

It is not, however, only the size of some species with the morphometric and coloration differences between young, immature and adult fishes that has caused confusion in the past. The colour of most groupers resembles that of their immediate surroundings and is accordingly variable within the species. Some species, however, assume colour patterns of vertical or oblique bands during sleep or anoxia, and these have in the past been assumed to be taxonomically significant. Another species, the coney, *E. fulvus*, has a very distinctive excitement colour phase which is totally different to its normal colour, and other species have highly distinctive deep-water and shallow-water colour phases (the deep phase is usually bright red). Not surprisingly, such variations in colour have been faithfully recognized in the past as taxonomically important and in the case of the black grouper (*Mycteroperca bonaci*) one nineteenth century worker gave this species six separate names each based on different colour phases.

Smith's revision removes confusions such as these, and is a major contribution to a better understanding of the groupers of the Americas. He also attempts a phylogenetic analysis of the species in the area and discusses barriers to dispersal and methods of transport available to this group. This study shows that although conventional taxonomic methods based on museum material may be difficult when the animals involved are large, when museum collections are supplemented by extensive field observations a clearer synthesis of the group is possible.

EXPERIMENTAL PSYCHOLOGY

Flashes of Brilliance

from a Correspondent

It seems that some more clues are emerging to clarify the connexion between intelligence and the electrical activity of the brain. Although electroencephalographic (EEG) techniques, in association with psychological tests, have been used successfully for some time to indicate brain damage and

mental retardation in patients, the precise relationship between the EEG and normal intelligence is still not clear. Conflicting evidence has been accumulating ever since Berger demonstrated in 1929 that the EEG could be used to monitor neural activity in the central nervous system.

Three years ago H. Weinberg (*Nature*, **224**, 813; 1969) showed that neural responses in the cortex (known as the visual evoked potential), as measured by the EEG, to brief flashes of light correlated with verbal intelligence; and more recently D. Shucard and J. Horn working at the University of Denver have found (*J. Comp. Physiol. Psychol.*, **78**, 59; 1972) that the latency of visual average evoked potentials (AEPs) also correlates with intellectual ability; thus long AEP latency was associated with low ability, and short latency with high ability.

To ensure satisfactory variance in both ability and AEP measurement, Shucard and Horn used subjects varying in occupation, age (16 to 18 years), sex and socio-economic status, and gave them a series of tests designed to measure different kinds of intelligence, including apprehension-span tests, visual speediness (as measured, for example, by the number of errors a subject makes when crossing out target numbers from a list of numbers), visualization tests, in addition to comprehensive measures of general ability. Subjects were later tested for AEP latency in several experimental conditions in which they were asked to respond to a light stimulus by pressing a button, simply by counting the flashes, or just by passively attending. Although correlation between these evoked cortical potentials and human abilities was significant, it was relatively small.

It was particularly interesting, however, that the correlation was greatest in conditions of low alertness rather than in conditions of higher enforced alertness, for example, when the subjects were pressing a button. These results support earlier work, for example, by Lansing, Schwartz and Lindsley (*J. Exp. Psych.*, **58**; 1959), and one interpretation of the data implies that the evoked potential represents a degree of intellectual arousal in which more intelligent subjects can maintain their alertness even during boring conditions, whereas duller subjects apparently cannot motivate themselves.

Shucard and Horn also point out that this ability of brighter individuals to regulate their state of alertness according to the task in hand agrees with the results obtained by Dinard and Defayolle (*Agressologie*, **10**, 525; 1969), although this interpretation does not altogether explain this latency-ability phenomenon.

Problems of Predicting Population

JOHN MADDOX

In the first of a series of articles on the environment, John Maddox discusses the factors and trends that will determine the future population of the world.

THE growth of the world's population is often held to be the mainspring of the environmental crisis. Since the Second World War, the pace of growth has been specially rapid. For the world as a whole, the population has grown from 2,400 million in 1945 to 3,632 million in 1970, the latest year for which the United Nations has produced an estimate¹. This corresponds to an average growth rate of 1.6 per cent per annum, which in turn implies that if the process of growth is exponential, the population of the world will double every 44 years.

One of the characteristic features of this recent population growth has been the difference between advanced and developing countries. For practical purposes, taking one country with another and one decade with another, the developed countries appear to have reached a condition of stability, at least as it must seem to the developing nations. The latter, however, have been growing quickly. On the United Nations classification of developed and developing nations², 31 per cent of the present population is in advanced communities and 69 per cent, the remainder, in developing communities. Thus in Europe as a whole, the average annual increase of population was 0.8 per cent a year throughout the 1960s, while in South-East Asia (where statistics are known to be unreliable) the corresponding rate is estimated to have been 2.8 per cent a year in the second half of the decade. In short, the developing nations have borne the brunt of population growth in the past three decades and the chances are that they will continue to do so in the years immediately ahead.

It is also plain that there can be no quick deceleration of

the rate of growth of population. People are people, after all. But even if great feats of Malthusian moral restraint were somehow accomplished by the putative parents of the developing world, the consequences of modern medicine have still to work their way through the populations of the developing (as distinct from the advanced) countries of the world. On present estimates^{1,2}, 85 per cent of the population of the developing world is under 45. Evidently there is a good chance that a large proportion of these people will be still alive at the turn of the century, a mere 28 years away (see Fig. 1).

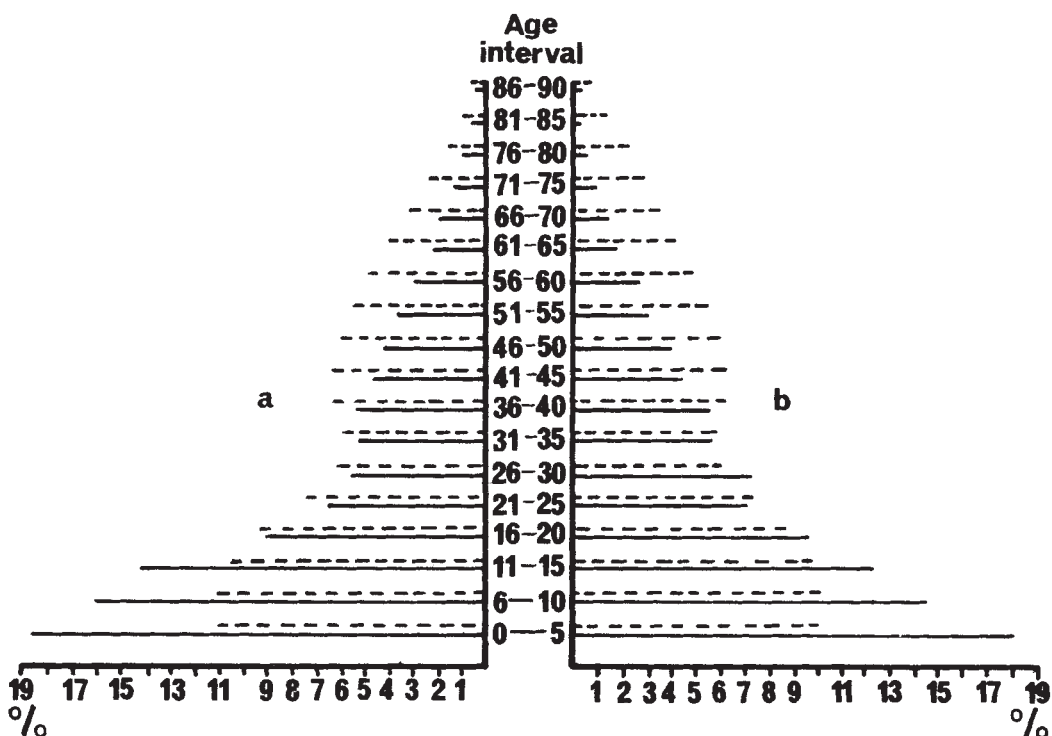
The difficulty is to know precisely what the total population will be in the years ahead, and whether it will grow to such a point as to create intolerable internal strains within the rapidly growing communities, for whom population growth and economic development are unpleasant alternatives, and whether there may even spring up physical shortages of food and mineral resources that will create insuperable difficulties in keeping civilization as such alive.

One immediate difficulty is in a sense historical. The present rapid growth of population in the developing world has come about because a rapid reduction of mortality in the years since the Second World War has not been matched, at least as yet, by a reduction of natality.

It is self-evident that such a state of affairs cannot indefinitely continue, for there are physical limits to the numbers of people who can be supported on the surface of the Earth. If population growth continued at such a rate that it doubled every half century, in less than 750 years there would be one person for each square metre of habitable land. More immediately, there are realistic limits to the potential size of the human population—food, crowding, the increasing cost of supplying raw materials.

Thus there is bound to be a time, at some stage in the future, when mortality and natality are so closely balanced that changes in the sheer size of the world's population can be comfortably contained. The practical question is not whether such a semblance of stability will be attained but

Fig. 1 Population distributions by age. ---, United States (1967); —, Madagascar (1966). a, Male; b, female. From ref. 5.



how soon and at what cost. Will the population of the world as a whole achieve stability by the comparatively painless methods which appear to have been successful in advanced societies, or will the Malthusian spectres of famine, pestilence and moral corruption (which he intended to include contraception but which must now be assumed to include the restriction of human freedom) supervene?

The incompleteness and inaccuracy of the population statistics is a serious impediment to forecasting. The United Nations Statistical Office considers that its estimate of world population in 1969 may lie anywhere in a range of 3.1 per cent which straddles the quoted figure, corresponding to an uncertainty of 113 million in the total. Several countries in the world do not carry out regular censuses. Others may do so but neglect to tell the outside world—an important consideration when it is recognized that mainland China is reckoned to account for something like a fifth of the present population of the world. But even where censuses are regular and methodical, errors can accumulate from one survey to the next, which is why a great improvement of the quality of demographic statistics can be expected as the results of the censuses of 1970 and 1971 become available.

For the prediction of future trends in population growth, head counts are insufficient. The potential for population growth is much greater in countries such as India, where larger proportions of the population are of childbearing age or younger, than in the older populations of advanced communities. But errors in estimating age distributions are invariably greater than estimates of total population. This implies that all attempts to predict the future population of the world start from initial conditions which are likely to be substantially in error. The refinement of the initial conditions is evidently an urgent need.

Given the age structure of a population and its composition by sex, the prediction of its size at some point in the future is in principle a straightforward stochastic problem. The future is determined by the present once there is accurate information about the probability per unit of time that males and females of specified ages will die, and probabilities per unit of time that women of specified ages will produce

children of specified sex. The probabilities concerned are usually represented by the age-specific mortality rate (conventionally expressed as the number of deaths per year per male or female of specified age) and the age-specific birth rate (expressed as the number of births per year per woman of specified age). The sex ratio at birth must also be determined. Essentially, these statistics must be derived empirically from whatever vital statistics may be available, which implies that future estimates of population size and age structure must depend on assumptions as to how these probabilities will change in the future.

In the nature of the problem, estimating future age-specific rates is as much an exercise in sociology as in statistics. For example, the extent to which mortality will decrease in the future is, for any population, an indeterminate function of the extent to which medical care will be made more widely available. Then the numbers of children born will depend on such intangible variables as the age at which women marry as well as on their child-bearing proclivities once married. Then there are variations, as yet not understood, between one part of a country and another which complicate predictions of the future. In Scotland in 1966, for example, the age-specific death rate among males aged 50–54 was 0.0147, compared with 0.00937 in England and Wales. Is this an intrinsic difference between the populations, the expression of a statistical bias (such as differential migration) or the result of different standards of medical care? Estimates of future age-specific death rates in Britain as a whole will not be precise until differences like these are understood.

Variations of age-specific birth rate with age provide a vivid illustration of the difference between advanced and developing communities. Fig. 2 shows age-specific birth rates for different countries. It will be apparent that developing countries stand out for the exceedingly high fertility of the younger age groups. The same figure also shows how fertility in Taiwan was reduced quite substantially between 1963 and 1968. Proportionately, the change is especially marked among women over 30. But even in comparatively developed countries, age-specific fertility rates can

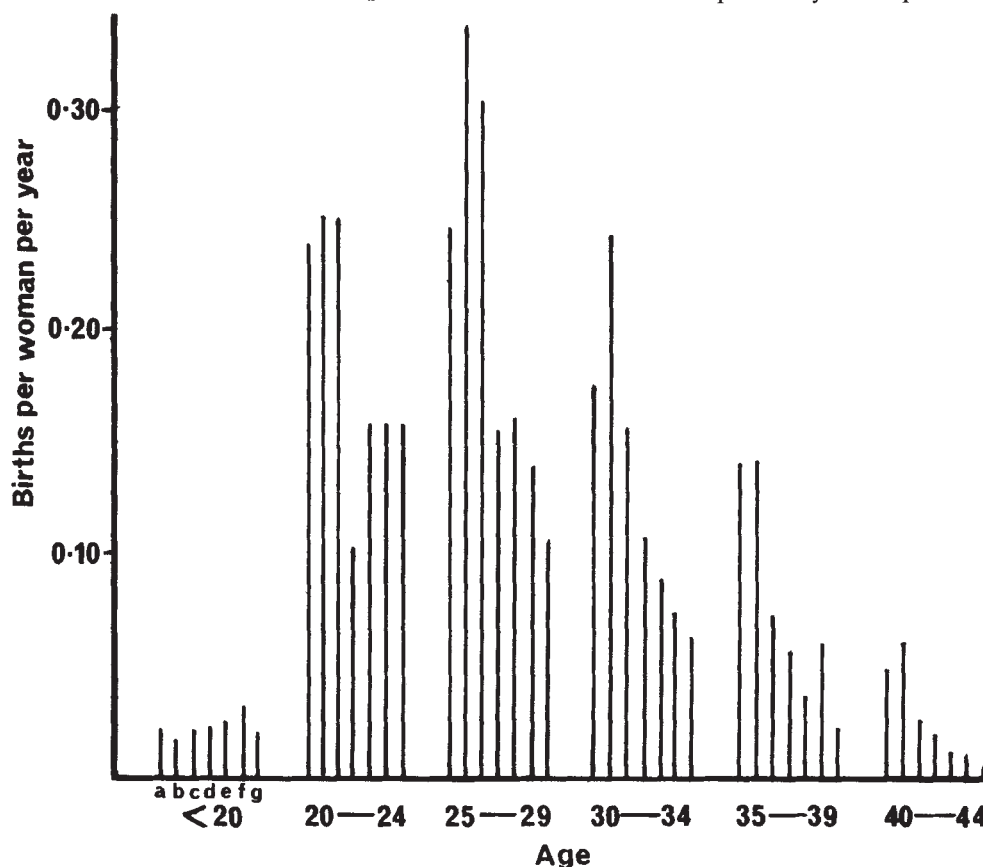
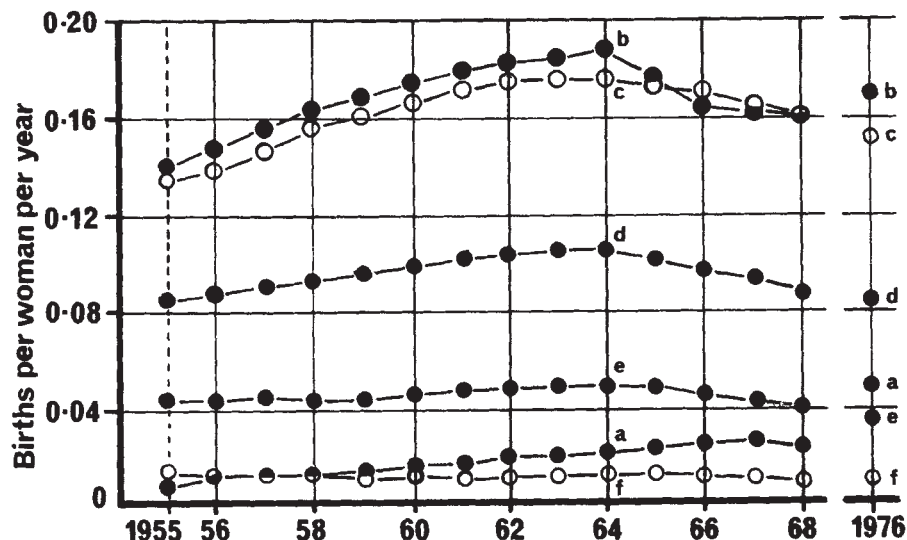


Fig. 2 Age-specific birth rates for different countries. a, Mauritius (1968); b, Taiwan (1963); c, Taiwan (1968); d, Italy (1967); e, England and Wales (1968); f, United States (1967); g, Hungary (1968).

Fig. 3 Variations of age-specific birth rates in England and Wales since 1955, and estimates for the corresponding rates in 1976. a, Less than 20; b, 20 to 24; c, 25 to 29; d, 30 to 34; e, 35 to 39; f, 40 to 44.



change dramatically in comparatively short periods of time.

Even in countries such as Britain, there may be considerable variations of age-specific birth rates in comparatively short intervals of time. Fig. 3 shows the variations of age-specific birth rates in England and Wales since 1955, together with estimates of the corresponding rates in 1976 which have been used as a basis for the predictions due to the Government Actuary⁴. It will be seen that the age-specific rates of the two most prolific age groups (20–24 and 25–30) have fluctuated by 25 per cent in 15 years. The rising trend in the early 1960s was the basis for the very large estimates of population by the end of the century that were current until recently. The assumptions made about age-specific rates in 1976 are an attempt to accommodate the recent trend towards early marriage in England and Wales.

In the circumstances, it is not surprising that demographic predictions of future population have been more often wrong than right. Fig. 4 shows four predictions of annual births in England and Wales put out by official sources in 1955, 1960, 1965 and 1970, together with the numbers of births recorded. The four projections span a range from 700,000 births a year to more than twice as many.

Fertility rates are, in present circumstances, much more volatile than death rates, at least in developed countries, which is why the forecasting of the older age ranges in a population is comparatively safe. For example, the number of people at present 28 or older in the United Kingdom is estimated by the Government Actuary to be 32.4 million. With conservative estimates of death rates in the years ahead, it is estimated that the number of people aged 28 or over at the end of the century, and who are by definition already alive, will be 34.87 million. In other words, largely because of the increase of longevity, the population of the United Kingdom will increase by 2.47 million between now and the end of the century. Similar attempts to forecast the size of the older section for developing countries are necessarily more hazardous, if only because the assumptions which must be made about death rates in the future are more doubtful.

Against this background, crude death and birth rates are chiefly of value as indices of historical change. According to the United Nations¹, the crude death rate in 1965–70 for the world as a whole was 14 per 1,000 per year, and the crude birth rate for the same period was 34 per 1,000 per year, which implies an annual rate of increase of the world population of 2.0 per cent per year. This is the most immediate source of present anxieties about population growth, but the crude birth and death rates, and the difference between them, conceal wide geographical variations as well as the extent to which the present rate of population increase is the product of increased longevity and excessive fertility.

Primitive communities were characterized by crude death

rates much greater than are found in modern societies, advanced or otherwise. Death rates often exceed 40 per 1,000 per year, but a measure of demographic stability would have been attained in consequence of a correspondingly high birth rate.

In such populations, the incidence of death is most apparent in the young age groups. Clark⁴ suggests, on the basis of archaeological evidence, that 55 per cent of the children of Neanderthal populations had died before the age of 14. By the early Bronze Age, the proportion living to 14 had increased to 50 per cent. Clark's compilation of survival curves for different populations suggests that the proportions of children living to puberty were no greater in China in 1930, among the slave population of the West Indies in the 1820s and among the Samburu community in East Africa as recently as 1958. Plainly, in circumstances like these, the survival of a community requires that for each surviving adult, potential parents must reckon on producing at least one other child for the grave.

The driving force for population growth in the developing world has been the rapid reduction of mortality. In many developing nations, the death rate is now much lower than at any time since records have been kept. In Ceylon, for example, the death rate in 1968 was 7.9 per 1,000 per year, compared with 11.8 per 1,000 per year in the United Kingdom. (This does not, of course, imply that the expectation of life in Ceylon is greater than in the United Kingdom, but merely that the Sinhalese population is younger.) Infantile mortality in Ceylon, however, is still uncomfortably large, at 47.7 deaths under 12 months per 1,000 live births, although still lower than elsewhere—in Zambia in the late 1960s, for example, 25.9 per cent of all newborn children appear to have died in their first year.

It is inevitable that birth rates should respond more slowly to changing circumstances, with the result that the countries now growing quickly are characterized by a death rate which may be historically low but with a birth rate which may be much higher. The result is an annual increase of population of as much as 3 per cent per year. In advanced communities, on the other hand, the death rate is low, of the order of 10 per 1,000 per year, and the birth rate may be substantially the same and certainly less than 20 per 1,000 per year. Table 1, taken from the Demographic Handbook for 1970, shows some examples of countries at present in three categories, consisting of those countries in which the death rate has fallen but in which there has so far been no corresponding reduction of birth rate (Group I), those in which the birth rate has fallen a little (Group II) and those which have reached the demographic pattern characteristic of advanced societies (Group III).

The question arises whether the condition which the

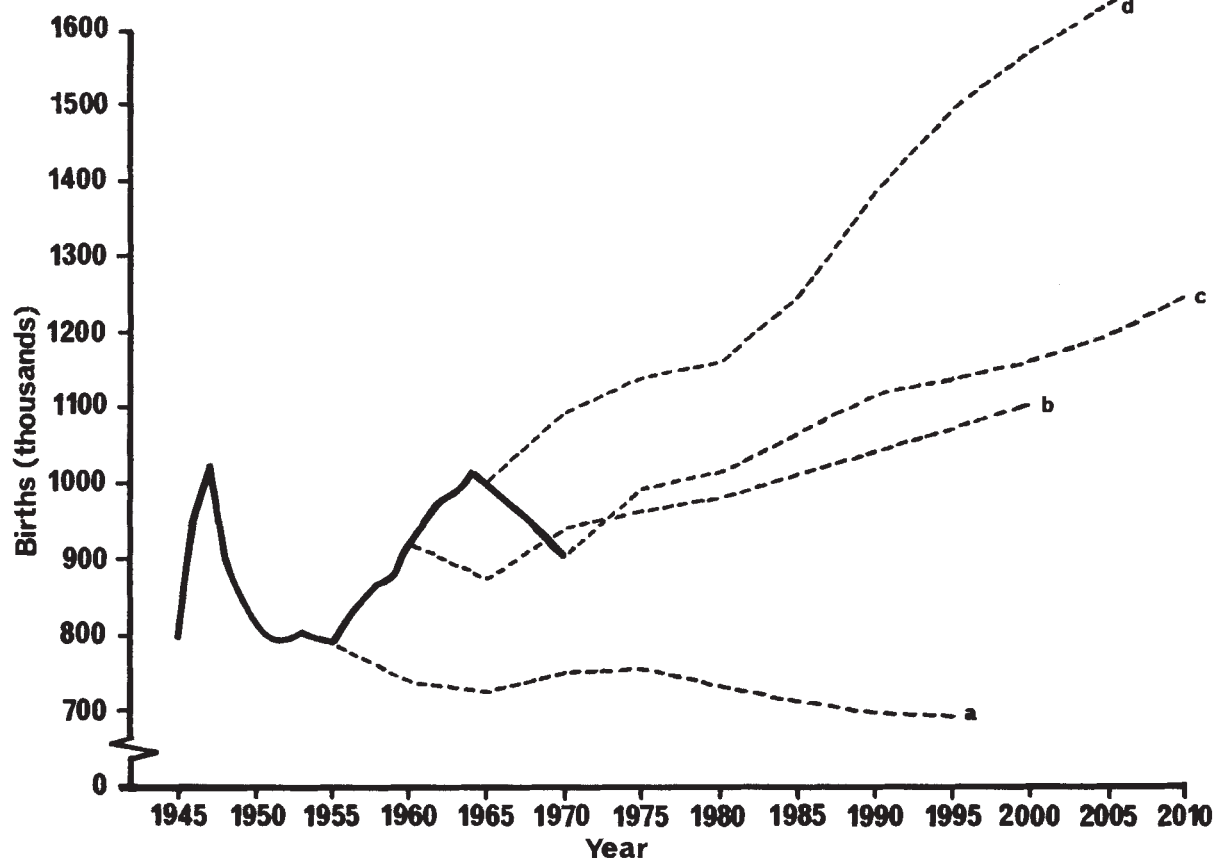


Fig. 4 Predictions of the annual number of births in England and Wales made in a, 1955; b, 1960; c, 1970; d, 1965.

advanced societies have attained corresponds to demographic stability of a kind. In the past few years, the notion of Zero Population Growth has been widely interpreted as constancy of population, which implies that increasing longevity should be offset by a reduction of the number of live births each year. In spite of the tidiness of this approach, however, other criteria of stability may often be less arbitrary. For many purposes, for example, it would be preferable to consider that a stable population is one in which the number of children born each year is for practical purposes a constant—then the provision of schooling for the young population will be less of an administrative headache.

In practice, however, the simplest criteria of population stability are statistical measures of fertility which are eventually indicators of whether one generation is replacing itself. One such is the total fertility rate calculated as the sum of all age-specific fertility rates for a population. The total fertility rate would thus be equal to the average number of children born to each woman if all women survived to the end of their reproductive period and if there were no variations of age-specific rates in the interval concerned. For practical purposes, the Gross Reproduction Rate, the same as the Total Fertility Rate except that it relates to girl children only, is a more meaningful statistic, but the neglect of mortality among women in early life remains a serious defect, which is why the Net Reproduction Rate is a more useful criterion of stability.

The Net Reproduction Rate is the sum of the age-specific fertility rates weighted by the probability that women will survive to different ages. In a population which is just barely reproducing itself, the Net Reproduction Rate should, of course, be 1.0. It is worth remarking that communities with the same Net Reproduction Rate may nevertheless have different characteristics. For example, countries in which children are born in rapid succession or in which childbearing begins at an early age will grow more quickly than those in which children are widely spaced or childbearing delayed, possibly by conventions of late marriage.

In the modern world, the advanced and developing nations are distinguished from each other and from those still in a primitive demographic condition by the large differences of Net Reproduction Rate. In recent years, several advanced countries have had Net Reproduction Rates of less than 1.0—in other words, generations have not been reproducing themselves. In Hungary, for example, the Net Reproduction Rate fell from 1.26 in 1953 to 0.82 in 1962 but has since recovered to 0.93 (in 1967)⁵. In Luxembourg, the Net Reproduction Rate has fluctuated about 1.0 since the early 1920s. In several European countries, the rate was less than 1.0 in the late 1920s and early 1930s—in Sweden, Britain, Belgium and France, for example. In the United States, the Net Reproduction Rate was 0.97 in the late 1930s but has since increased (by 1966) to 1.29.

The Net Reproduction Rate in developing countries is, of course, on present form, much larger, often exceeding 2.5. Nobody expects, however, that the high values of this index which now characterize the developing nations will persist. The question is how soon they will be reduced, and whether the reduction will go all the way to 1.0. The history of demographic change is an essential starting point for such a speculation.

In the communities which have achieved some kind of demographic stability, the reduction of mortality has been followed by a commensurate reduction of fertility. This process has now been well documented for several nations in Western Europe. In many countries, the reduction of birth rate has lagged a generation or more behind the reduction of mortality. Fig. 5 shows the variation of the crude birth and death rates in Sweden in the past 150 years. The death rate appears to have declined steadily since 1830, but for the first 60 years afterwards the birth rate remained in excess of 25 per 1,000 per year with the result that the population of Sweden increased rapidly. For most of the countries of Western Europe, the onset of the decline of birth rate can be placed at or near 1875, but there is some reason to think that in some countries—the Netherlands, for

example—the demographic transition is still incomplete.

Why should a decline of mortality be followed by a decline of fertility? And how have the nations of Western Europe managed to accomplish this feat without the benefit of modern techniques of contraception? Clark⁵ has argued that even a century and a half ago, most of the populations of countries which are now industrially advanced were not nearly as prolific of children as they might have been. After all, if in some communities each woman may on the average produce more than eight children in a reproductive lifetime, how is it to be explained that women elsewhere are much less productive? Social conventions which determine such things as average age of marriage have played an important part in regulating family size below the maximum breeding potential of a population. In England and Wales in the 1860s, for example, the average total fertility of women married in their teens was 8.4 births per lifetime, compared with 7.0 births per lifetime for women married in their early twenties. The convention of late marriage has had a more profound effect in regulating the population of Ireland, but elsewhere polygamy and polyandry worked in the same direction. And Clark argues that the way in which men were frequently required to be away from home in the United States in the nineteenth century had a powerful influence in restraining the growth of the population of nineteenth century America. It is striking that even in predominantly Catholic countries such as France and Italy, reproduction has consistently fallen short of reproductive capacity. In France, for example, total fertility has declined steadily from 5.0 children per reproduction lifetime in the late eighteenth century to 3.5 in the 1850s and to just over 2.0 at present. By one means or another, the populations of Western Europe have always been able to stay well within their full breeding capacity.

What are the prospects that the countries of the developing

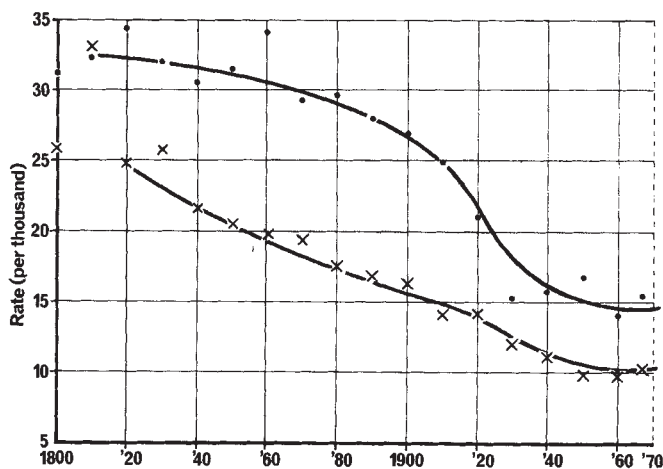


Fig. 5 Vital rates in Sweden. ●, Birth rate; ×, death rate.

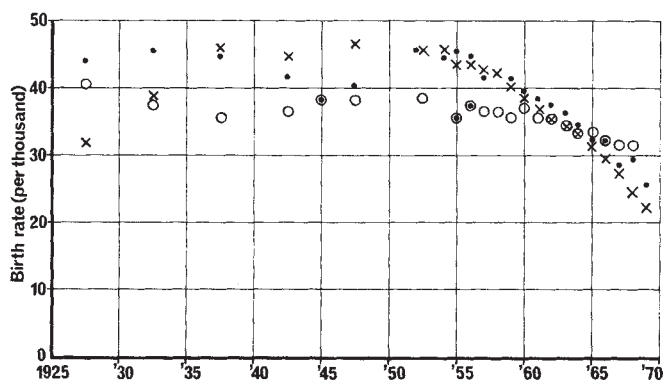


Fig. 6 Crude birth rates for some developing countries. ●, Taiwan; ○, Ceylon; ×, Singapore.

Table 1 Some Birth Rates and Death Rates

Group I	Birth rate (per 1,000 per year)	Death rate (per 1,000 per year)	Natural increase (% per year)
Algeria	49.1	16.9	3.22
Congo	44.4	22.7	2.17
Ethiopia	45.6	25.0	2.06
Kenya	47.8	17.5	3.03
Group II			
Barbados	20.5	7.8	1.27
Virgin Is. (UK)	26.5	8.8	1.77
Grenada	26.4	7.4	1.90
Trinidad	20.3	6.7	1.36
Group III			
Canada	17.5	7.3	1.02
USA	18.2	9.4	0.88
Japan	18.9	6.9	1.20
Singapore	28.8	15.9	1.80

world will be able similarly to match the dramatic reduction in mortality in recent years with a comparable reduction of fertility? The United Nations statistics show that in a great many developing countries, fertility reductions are already apparent. In Singapore, for example, the birth rate fell from 45.5 per 1,000 in the early 1950s to 22.2 per 1,000 in 1969. In Ceylon, the change has been less dramatic, from 38.5 per 1,000 in the early 1950s to 31.8 per 1,000 in 1968. Fig. 6 shows the way in which crude birth rates have changed in some developing countries in recent years.

The onset of the demographic transition has recently been considered by Professor Dudley Kirk, professor of demography at Stanford University⁶. Although the evidence is necessarily inaccurate, he detects signs that the decline of birth rate in those developing countries in which the demographic transition appears already to have begun is more rapid than in the demographic transitions which Western Europe and North America have experienced. Thus he represents the birth rate by a linear regression equation of the form $a + bt$, where t is time in years and a and b are coefficients. For the nations of Western Europe in which the demographic transition was for practical purposes completed between 1875 and 1940, the average value of the constant a , corresponding to the birth rate at the beginning of the transition period, appears to be 35.1 births per 1,000 per year, and the coefficient b appears to be 0.30 births per 1,000 per year per year. In Southern Europe (Italy, Portugal and Spain), the demographic transition appears to have begun later (in the mid-1880s in Italy and Spain and later still in Portugal) and is still incomplete, but the coefficient b works out at -0.25 , corresponding to a reduction of the crude birth rate by 2.5 each decade. In Central Europe, initial birth rates were higher (an average of 41.9 per 1,000 per year in the 1870s) but the pace of change has been faster with $b = -0.39$. In Eastern Europe and Japan, with comparatively late onsets of demographic change and comparatively high birth rates, the pace of change has been still faster, with $b = -0.44$. But in the developing countries which have recently entered the period of demographic change, however, the pace of change is faster still. Table 2 shows the calculated coefficients in the regression analysis for a group of these countries.

The questions raised by these calculations have a direct bearing on the likely course of events in the demography of developing nations in the years ahead. First of all, it does appear that once the demographic transition has begun, its pace may be more rapid than were the demographic transitions in the countries which have reached a condition of relative stability. There are, of course, exceptions. In Brazil, for example, the birth rate remains obdurately above 40 live births per 1,000 per year. This is why there will be in the next decade a close scrutiny of tendencies in countries such as India for signs of incipient demographic change. If the

hopeful experience of some developing countries is followed within the next decade by similar tendencies throughout the developing world, estimates of population by the end of the century may turn out to be too large.

The use of crude birth rates is, of course, far from ideal as a means of following the demographic transition. But statistics such as the Net Reproduction Rate which are standardized for the age distribution of the population and mortality up to the end of reproductive life are not often available, especially for the countries in which population growth is fastest. In Taiwan, however, it is known that the Net Reproduction Rate fell from 2.80 to 2.20 between 1956 and 1966 (while the crude birth rate fell from 44.2 to 31.9), that the Net Reproduction Rate in Singapore fell from 2.90 to 1.89 between 1957 and 1967 (while the crude birth rate fell from 43.2 to 26.0), but that in some of the intransigent populations of Latin America there has as yet been no sign of change—in Chile, for example, the Net Reproduction Rate was 1.64 in 1954, 1.87 in 1960 (largely because of a reduction of mortality), but still 1.68 in 1967. During this period, the crude birth rate in Chile fluctuated about 33.0 until the mid-1960s and has only recently begun to fall, but at a pace that suggests that a rapid demographic transition has begun.

The figures which are available for the onset of the demographic transition in various countries suggest interesting and important correlations with economic and social development. Kirk finds, for example, a negative correlation between birth rate and Gross National Product per capita. One especially important correlation is that between birth rate and the female expectation of life at birth, which is also markedly negative; social surveys within developed and developing nations have shown that total fertility of women is smaller among those social groups among which infant mortality is small.

What does this imply for the future population of the world? The Population Division of the United Nations is the chief source of continuing projections. The assumptions on which its current forecasts are based were defined in December 1969 (ref. 2). The most recent set of predictions¹, based on these assumptions, are improvements on previous calculations of this kind, in the sense that they predict not merely the total population of the several regions of the world but the age structure as well. In all these predictions, developed nations are dealt with differently from the developing nations. The former, the population of which in 1968 is assumed to have been 1,038 million, are assumed to be such a homogeneous group that they can often be dealt with as a whole. For the period between now and the end of the century, attempts have been made to extrapolate recent tendencies towards improved longevity, especially among women, and it has been assumed that countries in which mortality is at present greater than that in Europe and North America will catch up quite rapidly.

The assumptions made about the fertility in developing nations imply that the birth rate will decline only slowly between now and the end of the century (from 18.6 per

1,000 per year to 17.5 per 1,000 per year) which corresponds to an annual rate of growth in the developed world of 1.0 per cent per year between now and 1980, 0.9 per cent in 1990 and 0.8 per cent per year in the 1990s. With these assumptions, it is calculated that the total population of the developed world will increase from 1,090 million in 1970 to 1,545 million by AD 2000. Of this increase, 226 million will be in the age groups older than 25, most of whom are already born. The Gross Reproduction Rate is supposed to decline from 1.3 in 1970 to 1.2 in 2000.

The assumptions made about vital statistics in the developed world are conservative. They assume, for example, that there will be no general reduction of fertility such as that recently apparent in the United States, which accounts for 20 per cent of the developed world at present. In other words, given the volatility of fertility in advanced communities, this part of the United Nations prediction may be too gloomy.

Predicting the population of the developing world is still more difficult, because of the uncertainty which there must be about the pace at which fertility will be reduced. One set of predictions put out by the United Nations is based on the assumption that fertility will everywhere remain at the level of 1965 but that mortality will be reduced to varying degrees. With these assumptions, the population of the developing world would increase from 2,559 million in 1965 to 6,370 million in the year 2000, which implies that the total population by the end of the century would be 7,822 million. Because fertility is already falling, this estimate is very much an upper limit.

The United Nations Population Division has also published three alternative estimates of the future population of the developing world on the basis of different assumptions about the future decline of fertility. Each country of more than 250,000 has been dealt with separately. The most common assumption appears to have been that in the first five years after the onset of fertility reduction, the Gross Reproduction Rate would decline by 5 per cent, that it would decline by 10 per cent in each of the two succeeding intervals of five years and by 15 per cent in each of the three succeeding intervals of five years. This corresponds to a reduction of 50 per cent in the Gross Reproduction Rate in the first 30 years after the onset of fertility decline, beginning at a point determined separately for each developing country but assumed to be somewhere in the next 15 years. The prediction of this model, the medium variant as the United Nations calls it, is that the developing world will have grown to 5,040 million by the end of the century giving a population for the world as a whole of 6,494 million. At that point, according to this prediction, the population of the world will be growing at 1.7 per cent a year (compared to 2 per cent at present). The developing countries will still be characterized by the high birth rates and low death rates—the assumptions imply that the birth rate in the developing nations will by the end of the century amount to an average of 27.4 per 1,000 per year and that the Gross Reproduction Rate will be 1.7. The two other models, the high and the low variants, advance and retard the onset of fertility decline by five years.

Table 2 Some Calculated Coefficients in Regression Analysis

	Period	<i>b</i>	<i>a</i>
		(births per 1,000 per year per year)	(births per 1,000 per year)
Albania	1955–1968	–0.69	43.8
Ceylon	1948–1967	–0.42	40.9
Hong Kong	1957–1969	–1.44	39.2
Malaysia (West)	1956–1967	–0.96	46.1
Puerto Rico	1949–1969	–0.81	41.8
Ryukyu Is.	1950–1969	–1.15	37.8
Singapore	1949–1969	–1.21	50.2
Taiwan	1954–1969	–1.22	46.4
Trinidad and Tobago	1954–1968	–0.85	41.9

¹ *United Nations Demographic Yearbook, 1970* (United Nations, 1970).

² *World Population Prospects 1965–1985 as Assessed in 1968*, UN Working Paper No. 30 (United Nations, December 1969).

³ *Census 1971, England and Wales, Preliminary Report* (HM Stationery Office, 1971).

⁴ Clark, Colin, *Population Growth and Land Use* (Macmillan, 1967).

⁵ Keyfitz, Nathan, and Flieger, Wilhelm, *Population, Facts and Methods of Demography* (W. H. Freeman, 1972).

⁶ *Rapid Population Growth—Consequences and Policy Implications* (Johns Hopkins, 1971).

⁷ *World Population Prospects, 1965–1985 as Assessed in 1968*, UN Working Paper No. 37 (Population Division, UN Department of Economic and Social Affairs, December 1970).

Reinterpretation of the Invalidation of the Principle of Competitive Exclusion

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Ayala has demonstrated a stable co-existence of two species of *Drosophila* that violates the competitive exclusion principle as defined by the Gause-Volterra equations. His data are shown to fit a non-linear competition model and to satisfy a meaningful statement of competitive exclusion.

MUCH confusion has arisen from Francisco Ayala's convincing demonstration that two species of *Drosophila* can coexist in the limited resources of a closed laboratory environment^{1,2}. Having demonstrated that such stable equilibria are not modelled by the Gause-Volterra equations, Ayala rejected the principle of competitive exclusion.

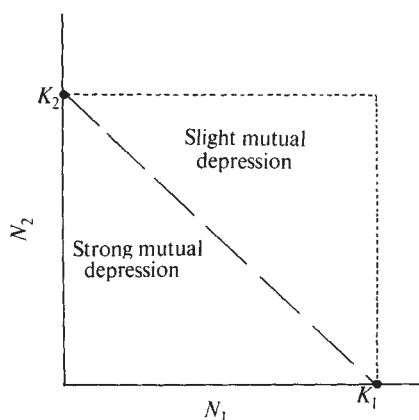


Fig. 1 Graphical definition of slight mutual depression. K_1 is the single species equilibrium for the first population. K_2 is the single species equilibrium for the second population. A straight line connecting K_1 and K_2 divides the plane into two areas: slight mutual depression, and strong mutual depression. For the Gause-Volterra model of competition, stable equilibria are only possible in the zone of slight mutual depression.

Answering Ayala, Gause³ insisted that the experimental environment contained two niches: a solid phase and an aerial phase, but Ayala has disagreed⁴, arguing that the *Drosophila* must compete in both phases. More importantly, Ayala shows that his experimental model violates a necessary condition that Gause and Witt⁵ derived for competitive coexistence—that coexistence is possible only with a “slight

mutual depression of species”. Ayala's equilibrium shows strong mutual depression, for the number of flies at equilibrium is less than would exist if either species inhabited the environment alone. In spite of their differences, Ayala and Gause apparently accept a common definition of competitive exclusion: “two species competing for limited resources can only coexist if they inhibit the growth of the competing species less than their own growth”⁴.

More recent experiments have led Ayala to believe that the equilibrium is maintained by frequency dependent fitness, and that the strong mutual depression of the populations results from interference¹.

Without attempting to explain the biological mechanisms that underlie this equilibrium, we shall create a model that resolves these mathematical and definitional difficulties.

Mathematical Models

The *Drosophila* systems Ayala studied clearly do not conform to the Gause-Volterra equations. For stable coexistence, this model requires that the total equilibrium population must be relatively elevated above the single species populations that could exist in the same environment. If the state of a two-species system is indicated by a point on a cartesian coordinate plane whose axes are the individual populations, N_1 and N_2 , then a stable equilibrium must occur above the straight line connecting the two single species equilibria, K_1 and K_2 (Fig. 1); this is the zone of slight mutual depression. Ayala's equilibrium occurs in the zone of strong mutual depression.

This lack of conformity to the Gause-Volterra equations does not directly imply anything about the competition between the two species. The Gause-Volterra equations are⁷

$$\begin{aligned} \frac{dN_1}{dt} &= r_1 N_1 \left[1 - \frac{N_1}{K_1} - \frac{\alpha}{K_1} N_2 \right] \\ \frac{dN_2}{dt} &= r_2 N_2 \left[1 - \frac{N_2}{K_2} - \frac{\beta}{K_2} N_1 \right] \end{aligned} \quad (1)$$

In the absence of competition, that is, where $\alpha = \beta = 0$, both equations reduce in form to the logistic growth equation. In the derivation of the logistic equation, the birth and the death rates are assumed to be linearly dependent on the population density. With *Drosophila*, neither assumption is valid⁸. For the birth rate, that is the recruitment of new adults, Pearl and Parker⁹ have observed, and Bodenheimer¹⁰ has confirmed, a hyperbolic decrease in recruitment with increased adult crowding. Thus, irrespective of competition, the Gause-Volterra equations could not apply to *Drosophila* systems. It is therefore incorrect to infer anything about the nature of the competition between *Drosophila* or the validity of the competitive exclusion principle from the lack of conformity to these equations.

In Ayala's experiments, the adults alone are enumerated. The earlier life stages are influential only in so far as they affect adult recruitment. Since Ayala is dealing with closed systems, the population, or density, of a species can be changed by but two factors: death and recruitment. This is expressed by the following mathematical identities:

$$\begin{aligned}\frac{dN_1}{dt} &= N_1 [b_1(N_1, N_2) - d_1(N_1, N_2)] \\ \frac{dN_2}{dt} &= N_2 [b_2(N_1, N_2) - d_2(N_1, N_2)]\end{aligned}\quad (2)$$

where d and b denote, respectively, the per individual death and recruitment rates, and where the subscripts identify the species.

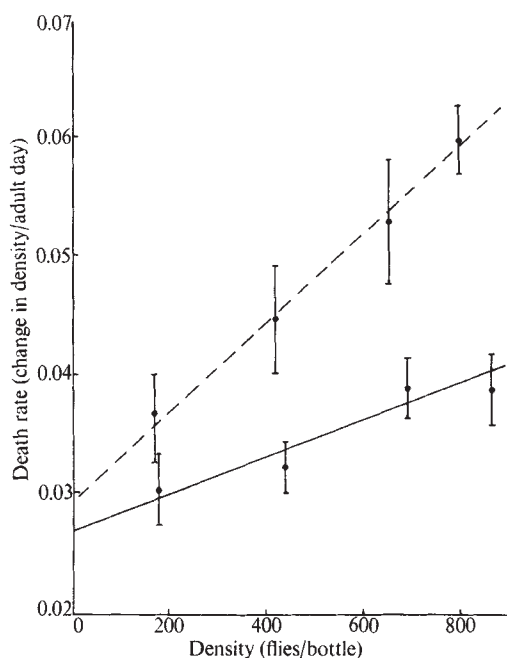


Fig. 2 Estimated functions for the intraspecific death rate. ----, Rate for *Drosophila willistoni* ($d_w(N_w)$); —, rate for *D. pseudoobscura* ($d_p(N_p)$).

These rates are functions of the densities of both species. We assume these functions to be continuous and single-valued. Further, we assume these functions to be separable over addition to the sum of two functions, each of which is a function of the density of a single species. Thus, the recruitment and death rate functions may be separated as follows:

$$\begin{aligned}b_1(N_1, N_2) &= b'_1(N_1) + b''_1(N_2) \\ b_2(N_1, N_2) &= b'_2(N_2) + b''_2(N_1) \\ d_1(N_1, N_2) &= d'_1(N_1) + d''_1(N_2) \\ d_2(N_1, N_2) &= d'_2(N_2) + d''_2(N_1)\end{aligned}\quad (3)$$

The primed functions give intraspecific effects; the starred functions give interspecific effects. If each of these functions can be determined, the complete dynamics of the system are then specified, including the existence of equilibria, stable or unstable.

Estimation of Functions

Ayala¹ provides the proper data from which to calculate these eight functions. In his competition experiments with *Drosophila pseudoobscura* and *D. willistoni* All, he also includes independent data on the death and recruitment rates, both for single-species cultures and for the two-species cultures.

These data yield, in effect, four points from which to estimate each function. A polynomial with four independently chosen parameters will fit four points exactly, but we reject this as unjustified. Instead we shall find the simplest functional curves that pass within the standard error of each point.

From the data on the single-species cultures, the primed functions of equations (3) are obtained (Tables 1 and 2). The survivorship data give weekly death rates. The daily death rate is assumed to be constant; it is obtained through the relationship

$$d_{\text{daily}} = 1 - (1 - d_{\text{weekly}})^{1/7} \quad (4)$$

The average density during the week is calculated, and in Fig. 2 each death rate is plotted against its corresponding average density, which allows the estimation of the individual death rate functions.

The average daily individual recruitment rate is obtained by dividing the week's production first by seven and then by the average density. Pearl and Parker⁹ have determined that the functional form of *Drosophila* recruitment is

$$b(N) = AN^B e^{CN} \quad (5)$$

Correspondingly, we have chosen values for the parameters A , B and C that best fit the curves to the data points (Fig. 3).

The starred functions, which give the interspecific effects, are equal to the general function minus the primed function, for example,

$$d'_1(N_2) = d_1(N_1, N_2) - d'_1(N_1) \quad (6)$$

Points from which to estimate the starred functions may be so obtained: wherever the general function is known for a species the primed function for that species is evaluated and subtracted. The starred functions should be zero at zero density, and this is also added as a constraint.

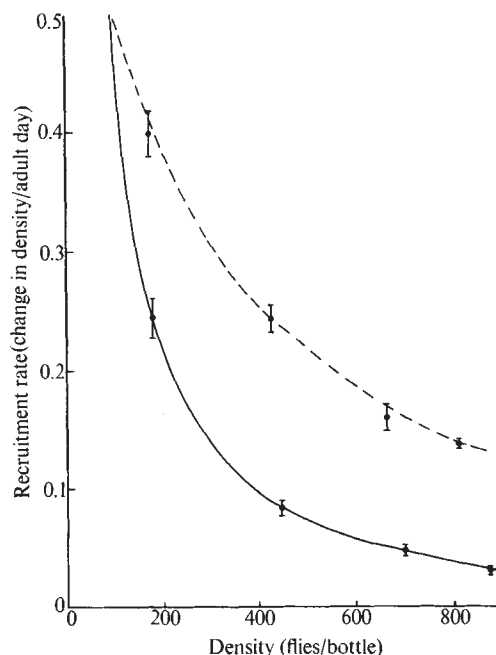


Fig. 3 Estimated functions for the intraspecific recruitment rate. ----, Rate for *Drosophila willistoni* ($b_w(N_w)$); —, rate for *D. pseudoobscura* ($b_p(N_p)$).

Table 3 shows the calculations of the individual death rates and the average densities for the two-species cultures. The intraspecific contribution to the death rate is removed by subtracting the primed function evaluated at the average density of the species. In Fig. 4 the remaining part of the

Continued on page 299

REVIEW SUPPLEMENT

Do Technical Reports belong to the Literature?

OVER the years, it has become painfully apparent that the writing of technical reports has been one of the most rapidly growing components of the scientific enterprise. Largely because of the growth of organizations such as the United States Atomic Energy Commission and its counterparts elsewhere and the proliferation of technical advisory committees within the framework of the specialized agencies of the United Nations and their offshoots, the International Maritime Commission, for example, increasing numbers of scientists have been increasingly concerned with compiling technical reports. Some of these are humdrum documents, reviews of the literature in some narrow field, reports on particular experiments or calculations more suitable for the backs of envelopes than for the solemn stationery in which they are distributed. Some, however, turn out to be important and distinguished contributions to understanding, and the question arises how these are eventually to form part of the scientific literature. Those responsible for learned and scientific journals are increasingly aware of one important aspect of the problem, for there is what seems to be a growing stream of complaints from the authors of technical reports that their priority for some new idea or experiment has been stolen by the author of an article in the more familiar scientific literature. Nobody suggests that plagiarism has run riot, but there is no doubt that the publication of original results or ideas in technical reports is a professional hazard for many scientists.

What is to be done? Many governments have now made arrangements to ensure that technical reports which can be published are widely known in the professional community. In the United States, for example, the Department of Commerce circulates complete lists of all the documents available to the public, and makes selections of these by subject for monthly distribution. In Britain, the old Ministry of Technology sponsored similar arrangements which have so far been less fully worked out. The United Nations has, by comparison, a long way to go in providing full information about the technical reports that are available. On the face of things, however, the principle seems to be accepted by most of the authorities concerned that the technical reports should be made widely available wherever such questions as national security are not concerned, and that, of course, is as it should be. Whatever the defects of this vast and growing output of scientific literature, at least there is no reason why it should not be distributed to all who want to read it.

That said, however, there is a great need that technical reports made available for public sale should conform with at least some of the principles which regulate the proper parts of the scientific literature. In the nature of things, it is too much to ask that technical reports should be refereed. Their function is often that of conveying

from a laboratory to its headquarters organization information on some matter of mutual concern. And where a technical advisory committee of, say, the World Health Organization has done its best to describe the best way of tackling some problem in public health, it would plainly be absurd to expect that some other body of advisers should sit in judgment on what is published. But it would be possible, and immeasurably valuable, to insist that literature like this should be carefully related to the rest of the scientific literature. References should be complete (and usually they are scrappy, to say the best of them). Nomenclature, systems of units and even the construction of the text should follow accepted conventions. And is it too much to hope that those responsible for publishing technical reports should see that first of all they are properly edited, with the commas in the right places and with the prose honed down to what is necessary?

There remains the problem of knowing how best to sift from the technical literature those parts of it which are original. The authorities responsible for making it available tend to take the view that that is a task for the original authors. Let them, the argument goes, decide what deserves to be published. The trouble, of course, is that authors are often slow to seize what opportunities exist. They also are ambivalent about the status of technical reports. Are they literature or not, and does the publication of the essence of a technical report in a learned journal count as an example of that crime against the literature, multiple publication? (The short answer is that it need not, at least if the author is quick off the mark.) That said, however, it is beyond dispute that piecemeal publication of the literature of technical reports in a more orthodox form would not be a sufficient substitute for active dissemination of its content among the scientific community. This is why it is essential that public authorities responsible for technical reports should also, as a public service, arrange that the essence of this material is also published in suitable summarized form, possibly as more easily available and regularly published volumes, ideally to be sold not through government departments but by ordinary publishers. Over the years, the scientific community has come to think that the sources of primary publication should be supplemented by a healthy review literature. The need is more apparent and more urgent for the literature of technical reports.

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General Books

Prehistory in Tanzania

Olduvai Gorge. By M. D. Leakey. Vol. 3. (Excavations in Beds I and II. 1960-63.) Pp. xix+306+41 plates. (Cambridge University Press: London, December 1971.) £10; \$32.50.

It is a great pleasure to welcome Dr Mary Leakey's long awaited and magnificent publication of the archaeological material from Beds I and II of Olduvai Gorge, Tanzania, in the third volume of the new Olduvai monograph series, edited by L. S. B. Leakey.

Serious archaeological work at Olduvai began as long ago as 1931, and the first general account of the archaeological sequence was published 20 years later. Many parts of it have long been drastically out of date, but such has been the pace of fresh discovery since it appeared, especially since the major campaigns of excavation financed by the National Geographic Society were begun, that the task of setting the published record generally straight has had to wait until these past few years, and can still only be carried out gradually. Indeed, the finds described in this volume are those from Beds I and II only up to the end of the 1963 excavations, though the study of them and the general conclusions reached are as up to date as the author could possibly make them; several details of post 1963 discovery are in fact reported, for example, in the list of hominid remains found at Olduvai and in Professor Richard Hay's section on the geological background of Beds I and II.

Olduvai Gorge is one of the outstanding prehistoric sites of the world, not merely of Africa or of the Palaeolithic period. The most spectacular finds so far have certainly come from the earlier levels, and are here described in detail for the first time. It is worth listing some of the highlights from Beds I and II briefly, to give the reader an idea of the scope of this volume, which will be the main source book for many years, even if it is to some extent an interim report.

Olduvai Bed I no longer has (as it did in 1963) the earliest dated occurrences of artefacts in the world, thanks to the work of the East Rudolf expedition, but it does have by far the best series of carefully excavated very early living floors. It has also produced a large and consistent series of chronometric dates, mostly by the potassium-argon method, many of them clustered around 1.75 million years. Hominid remains, including postcranial material as well as skull fragments and teeth, have been found *in situ* in Beds I and II, some-

times on living floors, in quantities which, by usual earlier Pleistocene standards, must count as abundant; their taxonomic range includes varieties of *Australopithecus*, *Homo habilis* and, from Upper Bed II, *Homo erectus*. Rich and immensely important faunal remains of all kinds occur throughout Beds I and II, and there is much excellent environmental and palaeogeographic evidence.

The stone tool assemblages are of outstanding interest in many ways, quite apart from their mere antiquity. Within the Olduvai tradition of so-called pebble-tool industries, certain developments and improvements through time can be demonstrated, from the lowest Bed I occurrences to those in Upper Bed II. The author distinguishes stages called Olduvai, Developed Olduvai A and Developed Olduvai B. In Middle Bed II there is the sudden appearance of a distinct biface tradition, with the Early Acheulian of site EF-HR, and this is carefully argued by the author as representing elements intrusive to the area rather than local technological development. Just as striking as this "vertical" variability of the Beds I and II archaeological assemblages is the "horizontal" variability within the Olduvai—that is to say, the differences in the composition of tool kits from site to site at any given stage. This is a well known phenomenon later on in the Palaeolithic, and it is fascinating to have it so well documented at this early period. The excavated sites have also produced information about early hominid activities far beyond mere tool making: the best examples will be found in the descriptions and floor-plots of the living sites and "kill" or "butchering" sites. The convincing structure of piled stones at DK (the oldest site in Bed I), the *Pelorovis* herd hunted down and butchered at BK (the highest site in Bed II), or the existence at MNK of a "chert quarry"—these are perhaps the best known or most spectacular evidence, but they are by no means the only items.

It is easy enough for a reviewer to list such highlights and add the occasional comment. That it is so easy presupposes the discovery of the sites, their excavation to a consistently brilliant standard, and the meticulous analysis of vast numbers of highly varied finds. It also presupposes that the resulting mass of data should be carefully organized for presentation in a monograph of exceptional clarity, with superb illustrations of the artefacts and admirable plans and sections. All of this is the real achievement of the author and her

colleagues and team, but especially in this case herself. There have been many archaeologists ready to criticize the slowness of definitive publication of Olduvai in the past. Let them study this volume carefully and consider how much has been achieved and presented; let them go to see the site for themselves and examine all the circumstances there; let them recall the ridiculously slender financial and technical resources available to the Leakeys for almost the first thirty years, and then ask themselves honestly how much more they themselves would have done. Not all may wish to return to the attack.

The main part of the volume consists of an illustrated account of the excavated Beds I and II sites, in stratigraphic order upwards, with detailed analyses of the industries, including percentage tables of various kinds, but no advanced statistical tests. The preliminaries to this account include definitions of tool types and a brief but important account by R. L. Hay of the geology of the two beds, including sections on stratigraphy, age, palaeogeography and sedimentation, the distribution of the archaeological material, and the raw materials used for tool making. The fold-out figures of correlated columnar sections for both sides of the Main Gorge, and for the Side Gorge, should please many users of this book, and the stratigraphy of the beds, the positions of the marker horizons, and the relation of the archaeological occurrences to the geological sequence will be found all now greatly clarified. There have of course been profound changes since the 1951 publication. Similarly, it is clearly stated in the final chapter how the current interpretation of the archaeological sequence from Beds I and II differs from the 1951 sequence of "Pre-Chellean" and "Chelles-Acheul" stages.

Following the main account of the excavated sites are brief chapters on the hominid remains and on the fauna from the living sites: these are subjects which will be reported on in detail in future major publications. A separate short chapter deals with the evidence for hominid modification of bone and ivory—five examples in Bed I and more than 100 in Bed II. A chapter of summary and discussion follows, setting out the main archaeological conclusions and commenting briefly on various aspects of their significance, with some reference to sites elsewhere in Africa. There are two appendices: one listing identified fauna from the various Beds I and II horizons, and the other, something of a bonus for the reader, briefly listing all the recorded archaeological

sites in the Gorge, from all the beds, with their nature, stratigraphic position, industry, date of discovery and date of excavation if excavated. Following the text are some admirably clear photographs, and tucked inside the back cover are several large fold-out plans and a section, including DK, FLK (Zinjanthropus site), FLK North and FLK NN.

The whole book, it will be seen, is thus essentially a straightforward and lucid exposition of a large body of archaeological data of outstanding interest and importance, together with an opening contribution, as it were, to the discussion which will last for many years. It certainly does not contain, nor is it intended to contain, the last word on any aspect of the specialist study or wider comparison of the finds. Much more remains to be done in a great number of ways, before it can be said that the finds from Olduvai Gorge Beds I and II have been fully described and understood in their complete African and world context. This could even take another forty years, and does not depend only on work at Olduvai. But what a foundation there is now on which to build!

DEREK ROE

A Shameful Tale

Mato Grosso: Last Virgin Land. By Anthony Smith. (An account based on the Royal Society and Royal Geological Society expedition to Central Brazil. 1967-9). Pp. 288 (200 photographs). (Michael Joseph: London. 1971). £5.

THIS is a sad book; it tells of the impending destruction of the largest area of primeval forest remaining in the tropics. The taming of this vast wilderness is inevitable under the pressure of growing human populations, but it is not being done with care after gaining the information necessary for conservative planning. The country is not being developed but exploited, with the aid of the internal combustion engine on wheels and on wings, in the most ruthless and thoughtless manner at the behest of absentee millionaire landowners anxious to grasp at yet more money. It is a shameful tale.

Some Brazilians have long hankered after a grandiose new Federal Capital—Brazil is a federation of United States—to supersede Rio de Janeiro as the seat of government. Now they have Brasília in the State of Goiás, a fantasia of “modern” buildings set in country until recently inhabited only by a few Indians and squatters. In 1965 the Brazilian authorities began building a road through the adjacent State of Mato Grosso as part of a great South American system of highways that was

to reach the Amazon River in the north; some of it is finished, but many thousand miles remain to be built.

The Brazilian Government invited several countries to send teams of scientists to study the territory near the new road while it was still unchanged. In response the Royal Society and the Royal Geographical Society sent a joint expedition, which set up a base camp 500 miles from Brasília at a place where the forest was practically undisturbed by man and his animals. In the course of two years forty-four British and twenty Brazilian scientists worked at the temporary field station, few of them for more than six months and many for shorter terms. Here, in a 20-kilometre-square sample area of the thousands of square miles of mato, the workers on zoology, botany, soil science, medicine, and geography carried out their researches, which in virgin country such as this were necessarily descriptive rather than experimental in the short time available.

Mr Smith, a journalist trained in zoology, was commissioned by the sponsors of the expedition to write this official account of the enterprises. It is based on three visits to Mato Grosso between 1967 and 1969, and gives an excellent picture of the country, its inhabitants, and of the day-to-day work of the scientists, with understanding, humour, and sympathy. Most of the visiting scientists not only carried out work of great value but enjoyed their experiences in this naturalist's paradise in spite of the usual insect and other discomforts of the undeveloped tropics. Although they could only scratch the surface, the information they collected is essential to developers and if it “is not heeded by them, disaster may befall not only the natural environment, but also the people concerned, be they peasants or labourers, capitalists or politicians”. Everything the author says about the exploitation of the country suggests that it will not be heeded, for the exploiters know nothing and care less about rational conservation and development. It was hoped that the base camp could become a permanent research station, but with changes of official policies and persons—Brazilian politics are ever unstable—which led to many earlier troubles, it had to be abandoned, “evacuated”, and left for the caboclos, the squatters liable to be dispossessed at any moment, picking over the pathetic rubbish to salvage what they could find.

The exploiters, who bought ranches of half a million acres or more, that were to them merely squares on a map, for the equivalent of a few pence an acre, seem to be the natural successors of the Indian Protection Service. The Service, which looked on the Indians as vermin, was dissolved in 1967 for the

wholesale slaughter of its protégés “by dynamite, machine guns, or sugar laced with arsenic”. The only redeeming feature of this dreadful story is the gallant work of the brothers Villas Boas among the Indians in the recently made Parque Nacional do Xingú. The outlook for the Indians is bleak: “The resistance of the formerly invincible tribes seems to disintegrate the moment their vulnerability has been proved. They would die without effort, or just vanish.” The picture on the book-jacket of the heir to the chief of a tribe reduced to fifty-three persons, lying listlessly in his hammock gazing into a hopeless future, too apathetic to brush away the fly on his arm, epitomizes the plight of the Amerindians.

The book is lavishly illustrated with photographs of the greatest beauty and interest, most of them in colour, taken by professional photographers specially commissioned for the work. Everything about this magnificent book, recording the results of British enterprise, is of the highest standard. It is printed in Japan.

L. HARRISON MATTHEWS

Early Eastern Europeans

Hunters, Fishers and Farmers in Eastern Europe: 6000-3000 BC. By Ruth Tringham. Pp. 240+12 plates. (Hutchinson University; December 1971.) £2.50 cased; £1.50 paper.

THIS book contains four sections: (i) the environmental background, (ii) post-glacial hunting and gathering communities, (iii) the earliest food-producers, 5500-3800 BC, and (iv) economic development and the earliest use of metal, 3800-3000 BC. These with the title indicate the scope of the book. The material will probably be unfamiliar to non-specialists, and has considerable intrinsic interest, so that the reader of *Nature* might be urged to take it up on that account; the principal part of its value for archaeology students lies in the descriptions and in the quite extensive references after each chapter. The main criticisms arise not from points of fact so much as from the nature of some of the interpretations offered, and the impression both kinds of reader might conceive of contemporary archaeology. Many people will not be satisfied with the explanatory value of “acceptance” against “rejection”, or “conservation” against “innovation”, on a background of diffusion. Similarly not many anthropologists will like a “change in village type caused by a modification of the social structure” (page 72), or by a “change in economy” (page 116), on the available evidence. The environmentalist may find the first section

sketchy, the "Elm Decline" more complex than is suggested even in passing (page 33) and the dates for the pollen zones unusual (page 42). For the archaeologist the theory of "cyclical settlement" in the Bandkeramik culture is evasively treated, and while in general it is useful to have the material marshalled in terms of the conventional cultural model it would have done to show some awareness of its deficiencies. Like the Mesolithic and Neolithic themselves it is retained in archaeology for its classificatory convenience rather than its explanatory value. But if the book is characterized more by conservatism than by innovation this does not lessen the fact that it is a valuable piece of work as a guide to the East European material. There is a great deal of information, description and references, which will be of value to archaeological students, while the text is sufficiently general in tone to reach a wide audience. The paperback edition in particular is excellent value for money, and a wide-ranging survey at this level was badly needed.

JOHN NANDRIS

Ball Lightning

The Nature of Ball Lightning. By Stanley Singer. Pp. ix+169. (Plenum: New York and London, 1971.) £5.85.

THIS is an excellent book. Stanley Singer has collected a formidable number of reports and theories on ball lightning and he presents these in a form which is both instructive and enjoyable. The book is well referenced; the list of nearly 600 published accounts forms an excellent bibliography in its own right.

The general features of ball lightning are first described on page 2 and are stated in slightly different form in the final chapter. If one combines these descriptions one might describe it as a luminous globe, frequently 25 cm in diameter, which occurs in the course of a thunderstorm, usually associated, although not necessarily directly in time or in location, with ordinary lightning. It often moves in a horizontal path at a low velocity and may last for several seconds or even minutes, disappearing suddenly with a loud explosion or without a sound. The author gives a minimum of comment and occasionally one could have wished that he might have questioned the veracity of some of the accounts. For example, the first two paragraphs on page 36 quote reports which are suspect. The first concerns the sighting of balls at 18 km which were 1° in angular diameter. These moved 10° in about half a minute. The author does not interpret this in similar

terms to the other reports. If one does, one finds that the balls are 300 metres in diameter and move at 300 km h^{-1} . They are probably an entirely different phenomenon. In the following paragraph a ball is described as the size of a cricket ball at 1.5 km. This is at or beyond the limit of resolution of the eye.

Stanley Singer makes it clear that all the theories to date are pathetically inadequate to account for the observations. Almost everything has been tried apart from the possibility of a macra-sized fundamental particle with a half life of one minute. Many theories founder on their inability to account for the stability of the phenomenon. Electrical discharge theories are discounted for their failure

to account for "some of the most striking characteristics, its motion, its appearance in enclosed spaces, and its failure to be affected by conductors". Plasmoids at first seem promising but they cannot be retained in the absence of self gravity or a similar, unknown, force. Chemical theories and those depending on vaporized metals do not fit the facts in the majority of observations. Radio frequency fields are shown to be more likely criminals but ducted waves which have been suggested to give a concentrated constant energy input can hardly explain the smooth and almost intelligent passage of the balls in airliners and many other observed locations.

The author concludes by suggesting that there may be many different types

Clothing

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The Science of Dress. [CHAP. VII.]

instead of hanging forwards as before, will incline backwards, his chest will grow broader and his back narrower, and his face bright and rosy with the improvement in circulation and breathing. On first standing erect he may feel somewhat stiff, owing to the previously unexercised muscles of the chest and back being brought into forcible action; the same feeling is experienced by adults, accustomed to stoop, when placed erect.



Fig. 4

A very pretty and nice dress for boys when breeched is the sailor's suit (see Fig. 4), which, by its looseness, allows free movement, is very durable, and covers all the limbs. The jacket must not, however, as is sometimes done, be cut low in the neck, or, if so cut, it must be filled up with a thick white flannel, so as to keep the warmth equal.

Under the suit flannel drawers and vest or combinations should be worn. Sailor suits should, however, be made with trousers, and may be worn till the boy is old enough to wear ordinary trouser suits.

The artist in mistake represented knickerbockers in the above illustration.

It is a pity that little girls as well as boys should not enjoy the freedom of Jersey knickerbocker and sailor suits; but prejudice is against this at present.

A page from Ada Ballin's *The Science of Dress in Theory and Practice*, 1885, reproduced in *Materials and Clothing in Health and Disease* by E. T. Renbourn, whose contents range from a chapter on the biophysics of textiles (by W. H. Rees) to a discussion on the psychology of fashion (H. K. Lewis, London, 1972; £9.50).

of ball lightning but if one analytically reduces the reports in his book one obtains a remarkably consistent account of the phenomenon quoted in the second paragraph of this review. The other reports concern ball-like apparitions but I wonder if they are really members of the family.

R. C. JENNISON

Higher Education

Any Person, Any Study: an Essay on Higher Education in the United States. By Eric Ashby. Pp. xi+110. (McGraw-Hill: New York and Maidenhead, May 1971.) £2.40; \$4.95.

THE Carnegie Commission on Higher Education, whose chairman is Clark Kerr and whose final report is due in 1973, has as its remit to examine the issues facing universities and colleges in the United States between now and the year 2000. The research studies it is sponsoring (some twenty-five have been published so far) are giving it a rich mine of new material; but it has also called upon several distinguished academics from outside the US "to describe and evaluate American education from their respective points of view". The first such commentary is this lucid and perceptive essay by Eric Ashby.

Here is indeed a book to ignite thought; shrewd in its recognition and identification of the problems of American higher education and in selecting those for particular examination about which he has penetrating things to say. But what we are given in effect is a realistic survey of the tasks for higher education which will soon be confronting us in Britain as well as those in the US—for the deeper the digging the more it is in common ground. We too are "at a climacteric of society when assumptions have to be revised".

In his treatment of such questions as who should be given higher education, what should be taught, who should teach it, and how research should be funded, Ashby has much that is pertinent to say. Is not a good deal of the work done for PhDs here too "competitive instead of contemplative"? How are we going to cope with the lack of motivation among some of those whom the nation needs to have highly educated? What is the proper content of a two-year course—whether leading to an AA, a BSc or a Diploma in Higher Education?

A few queries: this is a very masculine piece; would one gather from it anywhere that nearly a third of the students being talked about are likely to be women? Nor am I as sure as Ashby that a university's "own thing" is adequately covered by defining it, admirable though the definition seems, as "the pursuit of knowledge through

rational enquiry and discourse" unless "rational" and "discourse" are both widened beyond normal usage. An understanding of Mozart, Henry Moore, Corot, Psalm 139, the Bhagavadgita, the *Prelude* are highly rational occupations but the elemental activity of mind involved is not what is usually thought of as cognitive. I should maintain that to encourage the appreciation of any or all these works is very properly a function of higher education. To be humanely educated people need to retain experiencing power while adding to it critical power and among the deficiencies of university study today for very many is over-concentration on the latter at the expense of the former. Unless those who represent the university are widely and humanely educated how can they act as Ashby wants them to, as interpreters of their nation to itself—in other words its critics? For they have to recognize the educational needs of their society before being able to declare what they are and be possessed of some concept of the good life.

On the administrative side, Ashby boldly advocates a loosening of institutional bonds to give departments and colleges more autonomy. Perhaps there is no other remedy: but will enough consensus emerge to provide a really generous education for most students in civic universities and in polytechnics in our time? He says indeed less than he might have done (had he not rightly been concentrating first and foremost on the American scene) about relations between higher and further education. This may well be one of the key areas of difficulty in Britain in the next decade.

No more concentrated, wide-ranging and illuminating book about higher education has been published of late. It is profoundly concerned about the function of the university in the period ahead. Could we have a paperback edition? W. R. NIBLETT

Sentence Logic

Computer Logic. By Alan Rose. Pp. xii+180. (Wiley: London and New York, November 1971.) £4.

THE use of sentence logic to analyse relay and switching circuits was discovered by the American C. E. Shannon in 1938 and the Russian V. A. Rozenberg in the following year; landmarks in the modern development of the subject were the publication in 1951 of D. B. McCallum and J. B. Smith's paper on "Mechanized Reasoning—Logical Computers and their Design", and the 1956 volume, in the Princeton Annals of Mathematical Studies, of *Automata Studies*, edited by C. E. Shannon and J. McCarthy.

The author of the present work is well known for his extensive studies of

many-valued logics as well as for his successful applications of logic to problems in computer design. The book itself reflects the author's dual interests and his account of sentence logic is developed side by side with the practical applications of logic to the theory of decision elements.

On the purely logical side the main results established include the sufficiency of negation and disjunction for the generation of all truth functions, and the generation of negation and disjunction from each of the Sheffer functions, joint denial and incompatibility; the existence of the normal forms; and the conditions for a sentence composed entirely of variables, negations and equivalences to be a tautology.

Many and varied electrical models of sentence logic are discussed in detail. The basic element is the familiar make-and-break circuit of the electric bell, known as a relay. A particularly interesting example is the representation of conditional disjunction " $(P \text{ and } Q) \text{ or } (R \text{ and not-}Q)$ " by a relay in which Q is connected to earth through the coil and the switch moves between wires carrying inputs P and Q respectively.

There is an account of the Nottingham University logical computer designed by E. Foxley. An interesting feature of this computer is that it uses a decision element $\Phi(P, Q, R, S, U, V)$ from which each of the eight binary truth functions, disjunction, conjunction, implication, non-implication, equivalence, non-equivalence and the two Sheffer functions, are obtained just by a substitution of constants for two (or four) of the variables; $\Phi(P, Q, R, S, U, V)$ is in fact the conditional disjunction with P and V as first and third terms and with middle term the conjunction of the disjunction of Q and R and the incompatibility of S and U .

Perhaps the deepest section of the book is an account of work which the author did for English Electric in connexion with the design of a reactor for a nuclear power station. The problem was to find a control mechanism which would rotate a wheel through an angle which was a multiple of π/n , a multiple (positive or negative) determined both by the momentary position of the wheel and a given input number, together with certain safety requirements.

The author's aim was to write an introductory account of the subject suitable for those intending to work with computers but who have no previous knowledge of mathematical logic, and he has been highly successful. The mathematical prerequisites for reading the book are very modest, little more than a familiarity with symbolic notation and mathematical induction, and the book could be read with profit by an intelligent science sixth-former. R. L. GOODSTEIN

Biological History

Biology in the Nineteenth Century: Problems of Form, Function and Transformation. By W. Coleman. Pp. vii + 187. (Wiley Interscience: London, November 1971.) £3.50 cloth; £1.75 paper.

THE author, formerly at Johns Hopkins, now at Northwestern University, has already published a biography of Cuvier and a collection of texts on the interpretation of animal form. Like these the present work bears the mark of careful and sober scholarship. Belonging to the Wiley History of Science Series, *Biology in the Nineteenth Century* is aimed at the educated reader for whom it is claimed there exists no readily available work by scholars in the field.

Coleman has wisely adopted a severely selective treatment of his subject matter—nineteenth century biology—with the result that in a mere 166 pages one does get an impression of the trends within the subject and one can discern a major theme—the growing domination of many departments of the life sciences by the comparative method and the supplementation and eventual supplanting of this approach by the experimental method at the end of the century. The assumption of the comparative method, that all features of organisms are to be understood in terms of a single developmental process, led to the explanation of embryological development in terms of phylogenetic history, of which ontogeny was held to be a recapitulation, and to the placing of extant primitive cultures in a single evolutionary series. Coleman draws attention to the affinities of this method with the basic assumptions of unity and development in the writings in the German tradition of *Naturphilosophie*. In chapters 2–5, on the cell theory, embryology, transformism, anthropology and sociology, the comparative method is seen in action. In the last two chapters some aspects of physiology are examined, and this leads on to the re-introduction of the experimental method into descriptive subjects like embryology and variation. There is a dash of institutional history in the first and last chapters which give the lie to those who believe historians of science are old-fashioned “internalists”.

The selection of material is admirable; embracing “soft” as well as “hard” sciences it does not suffer from the distortion of a commitment to the reductionist programme. One might quarrel with the author's interpretation of some points—the so-called origin of Schwann's error over the cell-forming fluid in the theory of Schleiden, the influence of Darwinism on the early anthropologists, Morgan, Lubbock and Tyler, and on the supposed abundance

Architectural Environment



A house designed by Frank Lloyd Wright and built in Pennsylvania in 1935. This illustration, from the second edition of *American Buildings*, by James Marston Fitch (Houghton Mifflin, Boston, 1972; \$15.00), “tells us nothing about conditions inside where cascade is invisible, perceivable as noise, humidity chill”.

of geological time for the biologists of the Darwinian era—but as an introduction to the history of nineteenth century biology this book with its excellent bibliography is unequalled. The more is the pity that Coleman has not yet developed a more natural and flowing style.

ROBERT OLBY

Climate

Concepts in Climatology. By P. R. Crowe. Pp. xx + 589. (Longman: London, October 1971.) £6.

THE atmosphere is a great heat engine whose energy source is radiation from the Sun and whose energy sink is radiation into space. The unequal distribution of the effects of these two streams of radiation generates temperature gradients and forces the air into motion on a variety of interacting scales, from that of the globe itself to scales of a few centimetres found in intense turbulence. Some of the energy is used to evaporate water at the surface of the Earth, the energy subsequently being released through condensation in clouds, and precipitation of the resulting liquid.

All these important processes are discussed by Professor Crowe in his book: the geometry of solar radiation, terrestrial radiation and its transfer, the formation of rain, jet streams, fronts and evaporation. Relevant and interest-

ing statements are made about them all, although the commentary is marred at times by the author's over-anxiety to avoid technical jargon. As an example, the scattering of light may be a rather difficult concept, but to say that radiation is “tossed back” into space is hardly an improvement. This apart, the geographer for whom this book is intended will find much of interest, particularly with respect to the geographical distribution of surface variables but with the essential three-dimensional structure of the atmosphere well emphasized. But Professor Crowe's intended reader is no scientist, as he states quite clearly in his introduction, where he says that . . . because ideas have evolved, . . . “a comparatively historical approach is just as appropriate as the more mathematical-physical techniques employed by specialist meteorologists”. Now I believe that the evolution of ideas is an interesting subject but meteorology, which I take to embrace climatology, would be a poor subject if there were not the occasional breakthrough which caused us to re-think our historical framework. One immediately thinks of two recent discrete jumps of this sort. Computers and numerical models of the atmosphere have shown that the motion is determinate and in response to what are basically simple laws of physics. Again, satellite pictures have shown us

the beautiful symmetry and structure of the larger scales of motion. There is no mention of such models, and the only satellite picture appears, virtually without comment, on the dust cover. It is a picture of the southern hemisphere and all the cloud-whirls go the opposite way round to those in the northern hemisphere most of us are used to. It hits one as a left-handed thread does, yet it is hardly mentioned.

Many interesting facts upon which scientific inference is based are here, many concepts related to their interpretation are named, but for a scientist there is too little critical analysis of the value and usefulness of those concepts.

J. S. A. GREEN

Patent Problems

A Handbook on Patents. By H. W. Grace. Pp. viii+185. (Charles Knight: London, December 1971.) £3.

EVER since the publication of C. Lees's book *Patent Protection: the Inventor and His Patent* in 1965 with its inspired and readable tale of the invention of a potato peeler by a Mr I. N. Genius, there has been a need for a more up-to-date introductory volume to the intricate world of patents. The chief obstacle in this field has always been the legal phraseology in which the relevant laws and their interpretations are couched and which is usually wellnigh incomprehensible to those people who most need

some understanding of it, viz. the technical managers, research scientists, technologists and company secretaries whose activities may be affected by the existence of patent protection. The bridging of this language gap is very important in rendering this subject of industrial property into an acceptable frame.

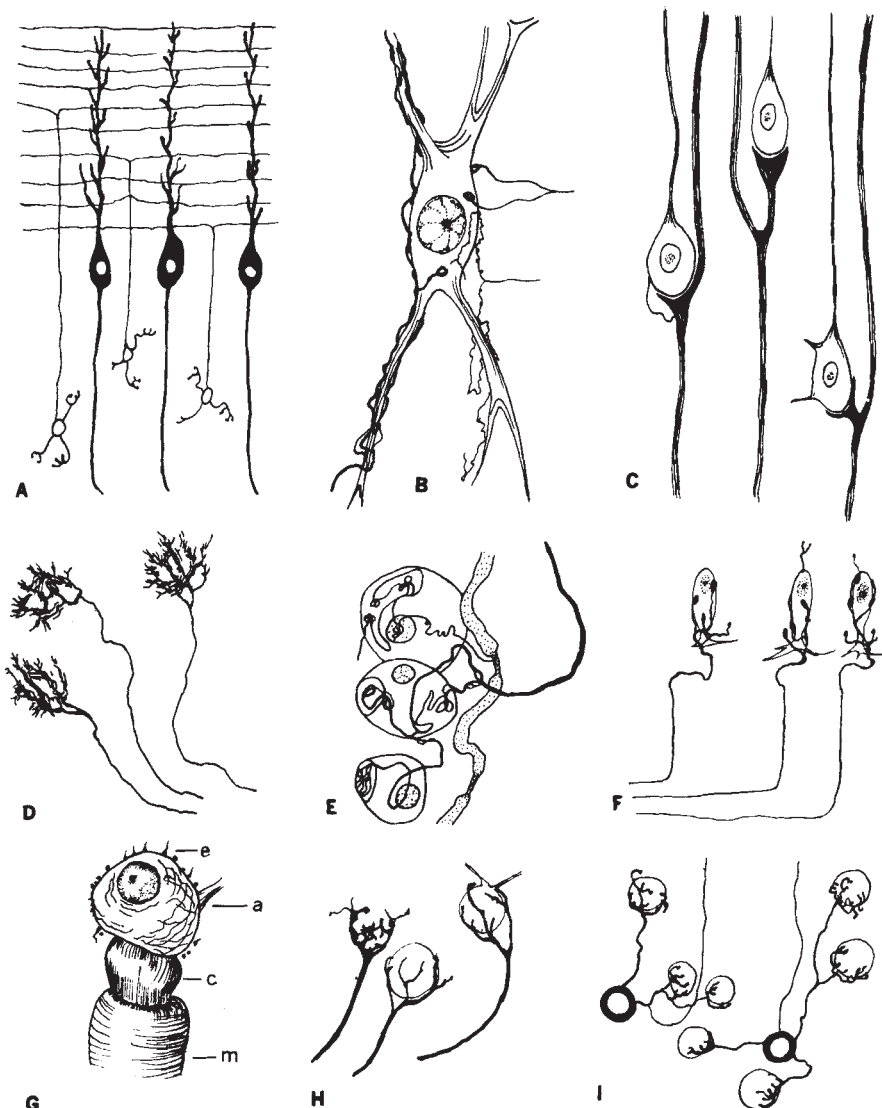
In the words of the publishers, this book is "primarily a book of ready reference to the basic principles and practice of patent laws for use by those involved with commissioning the filing and prosecution of patent applications at home and abroad, and with dealing in that class of industrial property". Yet this handbook, based on a series of lectures, is still written in *patentese* whose very existence and use make the topic so difficult to grasp. It is admittedly not so easy to present a good survey on the patent world without using the jargon of its trade, but it can be done as Lees has shown in his earlier book.

The present author has certainly succeeded in producing a competent survey of the British patent system with some brief references to the systems of the major industrial countries (except the USSR and the countries of Eastern Europe). A synopsis of foreign patent laws is given in a shorthand form in an appendix. Much of the information is refreshingly up-to-date, such as the reference to the new Japanese regulations of 1971 and the recommendations of the Banks Committee, although the knighthood conferred on its chairman must have come too late for inclusion in the book.

The book is well produced though some minor inaccuracies were noted, for example, the dating of the Statute of Monopolies on p. 1 which is generally referred to as being of 1623, or the spelling of the new German patent publications which should read "*Offenlegungsschrift*" on p. 33. A glossary of more than 60 terms of *patentese* is a very useful addition to this slim volume.

FELIX LIEBESNY

Communicating Cells



Illustrations of synaptic morphology taken from various sources: a drawing from one of the contributions to *Concepts of Communication: Interpersonal, Intrapersonal and Mathematical*, edited by Edwin F. Beckenbach and Charles B. Tompkins (Wiley, New York, 1971; £9.50).

Mediaeval Physics

Physical Science in the Middle Ages. By Edward Grant. Pp. xi+128. (Wiley: New York and London, November 1971.) £3.50 cloth; £1.75 paper.

THE title of this book is rather misleading, for it suggests a wider coverage than is in fact given. Professor Grant has realized the great extent of the material before him, and has decided to concentrate on the Aristotelian tradition in the later Middle Ages. This immediately means that we are told virtually nothing of the work of such men as Grosseteste, Jordanus, Petrus Peregrinus, Roger Bacon and Witelo. Grant's first twenty-

six pages summarize the state of science in the early Middle Ages, the activity of the translators from Arabic and Greek, the growth of universities, and the early impact of Aristotle. He then considers the Bishop of Paris's condemnation of 219 propositions in 1277. Many of these propositions were connected with the Aristotelian tradition, and often they had the effect of limiting God's freedom. Grant sees the condemnation as having a pronounced impact on subsequent discussions, and relates it to the later mediaeval habit of considering what could happen in a possible world. The fourth chapter is the best in the book, and gives an admirably clear account of fourteenth-century discussions of motion, the fifth chapter centres on fourteenth-century cosmological discussions. Both of these chapters draw on much recent work, includ-

ing Grant's own. In his conclusion Grant argues persuasively that one of the main differences between a man like Copernicus and his fourteenth-century predecessors lay in the belief of the former that he had arrived at physical truth and not just a plausible hypothesis. In his final sentence Grant writes of Copernicus's work: "This is the stuff of error, fantasy, and scientific revolutions". The book includes a long and valuable bibliographical essay, which covers far more aspects of mediaeval science than does the text or even the title.

Anyone considering the history of mediaeval science must from time to time ask himself what falls within his ambit, and in particular, in the context of the Aristotelian tradition, how if at all "science" is to be disentangled from philosophy. Grant seems simply to assume (for example, on p. 69) that a clear

distinction is possible, but does not give any grounds for it. It seems to me that in selecting his topics he has relied mainly on analogies in subject-matter with later discussions that are unambiguously dubbed scientific. Such a principle of selection may be necessary, but it is anachronistic, and so should be clearly stated. The attempt to separate so rigorously science from philosophy may even have had an ill effect on Grant's fine chapter on motion, where clarity of expression is partly obtained at the price of disguising from the reader the richness, and sometimes the tortuousness, of the primary materials.

Provided its limitations are recognized, this will be a very useful book. Its worst feature is its price: few students or general readers will wish to spend so much money on so few pages.

A. G. MOLLAND

Physical Sciences

Lunar Geology

Moon Rocks and Minerals. By Alfred A. Levinson and Ross S. Taylor. Pp. xiv + 222. (Pergamon: Oxford and New York, 1971.) \$11.50.

Professors Levinson, editor of the Apollo 11 Lunar Science Conference (LSC) Proceedings, and Taylor, member of the Lunar Sample Preliminary Examination Team, are well qualified to attempt to reduce a million or so words of the published LSC (held at Houston in January 1970) to some 50,000 words; and they have been eminently successful in this task. Anyone who attended the first Houston Conference or who has read the collections of specialist reports (*Science*, 167, January 1970) or the corresponding papers (*Geochimica et Cosmochimica Acta*, Special Supplement 1, 1970) on the Apollo 11 rocks will be aware of the difficulty facing anyone who endeavours to cross the frontiers of the numerous disciplines involved and to interpret and assess the overall significance of the gamut of results presented there. Individually, the published papers are fascinating. Read as a whole it becomes evident that the very important element of cross-fertilization of concepts is generally missing; and that is why a clear attempt to collate the varied information about the Moon and generate an understanding of the Apollo rocks is to be desired.

Before examining this book, I felt that any work dealing with the Apollo 11 rocks was bound to be premature in the sense that less risky, better balanced, conclusions could have derived from the results of analysing all the rocks returned following the

completion of the whole Apollo programme. The authors have presented the data well (there are numerous tables, graphs and illustrations of rocks, including thin sections, several in colour) in a refreshingly concise yet generally well-written manner; the authors have been aware of, and presented the reader with, just enough of the Apollo 12 preliminary results to effect a cautionary approach to generalities based predominantly on the data drawn from one site alone, and the net result is that one sees a definite utility in this book.

The planetary scientist, or geologist with little knowledge of the Moon, will find *Moon Rocks and Minerals* a convenient guide to the principal geological results drawn from Apollo 11. Contrary to the impression conveyed in the publisher's blurb, the layman with no knowledge of geology or chemistry will hardly understand the contents. He might wish to consider buying the slightly cheaper book *The Lunar Rocks*, by Mason and Melson, as an alternative source of information about the Apollo 11 rocks; the latter book is somewhat less critically argued but, possibly, the layman will find it more readable than the book under review. *Moon Rocks and Minerals* contains a glossary of terms which the non-geological planetologist will welcome, but the subject index is inadequate. No reference lists are given, a reasonable omission if one accepts the view that the interested scientist will probably read this book with the full, published papers close at hand for detailed reference.

The thinking in both books is pervaded by the underlying assumption that mare basins were created by impact and, whilst this view is now held

generally, the level of science in the books would have been more in keeping with that of the critical, objective approaches applied through studies of the geology of the rocks had such an *ad hoc* assumption been avoided—or, at least, had the arguments for and against it been given. For example, Levinson and Taylor present two hypotheses of mare formation in both of which the formation of a basin by impact goes unquestioned (apparently without any direct support for the impact process); on page 163 it is said that the Apollo 11 ages from Mare Tranquillitatis date the time of filling of the maria [*sic*] basin, yet it may be argued equally well (or better) that the observed spread of ages of the crystalline rocks from the Apollo site dates at least two particular, late flows which are not representative of the average time of flooding of the area (was it really a basin?). The authors do not mention that some investigators have suggested that certain of the lunar glassy spherules are of volcanic origin.

Levinson and Taylor have made little or no attempt (although this is possibly consistent with their aim) to master arguments other than quasi-geological ones. A physical argument (pp. 116, 198) for the rigidity and coolness of the Moon is assumed to be correct and opposing views are not explained; basalts (as the authors have shown elsewhere) would be expected to melt at temperatures somewhat higher than the quoted $\sim 1,200^\circ\text{C}$ (p. 157) in "the lunar interior"; physical (and other) arguments for the youth of Tycho are ignored, and sections on the capture and fission hypotheses of the origin of the Moon describe the chemical arguments to the

exclusion of the equally important physical ones. The explanation of the Roche limit, in a footnote on page 195, is incomplete and misleading.

Whilst the chapters on the physical properties, and the origin, of the Moon are disappointing (especially now that the reader is able to view them in the light of the results from the more recent Apollo missions), these chapters are short and do at least present a record of the more important geochemical evidence as we knew it at the time of Apollos 11 and 12. With these limitations, which in my opinion are quite acceptable in view of its title, the book is definitely to be recommended to the lunar and planetary scientist—who may well look for a revised edition and for follow-up texts concerned with subsequent Apollo missions. G. FIELDER

Corrosion

Advances in Corrosion Science and Technology. Vol. 1. Edited by M. G. Fontana and R. W. Staehle. Pp. x+347. (Plenum: New York and London, 1971.) £10.50.

THIS is the first of a series of review papers on selected topics in the field of corrosion, and it is anticipated that further volumes will appear at yearly intervals. The concept of "corrosion" as confined to metals and to reactions that are undesirable is refuted by the editors, who point out that the term will be interpreted to include all reactions of metals, glasses, ionic solids and polymeric solids with their environment; the series will include contributions from workers in fields peripheral to corrosion, since studies in the field of physics, physical chemistry, physical metallurgy, and so on are often of great value in the resolution of corrosion problems.

There is an excellent review article on "Techniques for the Measurement of Electrode Processes at Temperature above 100° C" (D. de G. Jones and H. G. Masterson), and in this connexion it should be noted that valuable and fundamental work in the field of high-temperature aqueous electrochemistry has been stimulated by the need to solve practical corrosion problems that arise in the electrical power industry; thus although workers in other disciplines contribute to the understanding of corrosion mechanisms, the converse is also true. The authors outline the various difficulties in obtaining useful measurements at elevated temperature and pressures, and discuss practical details such as design of pressure vessels and liners, methods of insulating and sealing electrode leads, types of reference electrodes, and so on that have been used by workers in this field. Examples are pro-

vided of electrochemical studies of ferrous and non-ferrous metals at elevated temperatures, with particular reference to corrosion problems that are encountered in electrical power generation and in high pressure desalination plants.

"Surface- and Environment-Sensitive Mechanical Behaviour" (R. M. Latinis and D. R. C. Westwood) reviews the influence of surface structure and environment on the mechanical properties of inorganic crystals, with some emphasis on metal monocrystals, and its inclusion in the book exemplifies the very broad definition of corrosion adopted by the editors; stress-corrosion cracking has been omitted because the authors consider that the subject has been dealt with adequately in a recent publication. The authors consider first how the nature of crystal surfaces differs from the structure of the bulk material, and conclude that the near-surface region may be regarded as a separate phase that ranges from a few atom layers in terms of structural rearrangement to probably microns in terms of chemical and electronic properties. They then proceed to give examples of environmental effect which by influencing the surface also affect bulk properties.

The control of corrosion by organic inhibitors is considered in the review paper on "Mechanism and Phenomenology of Organic Inhibitors" (G. TrabANELLI and V. Carassati), which deals with both the theory and practical application of inhibitors. There have been a number of attempts to correlate the structure of organic compounds with their inhibitive action, and the authors have reviewed the significant work in this field in some detail. Inhibitors are usually specific in their action, and

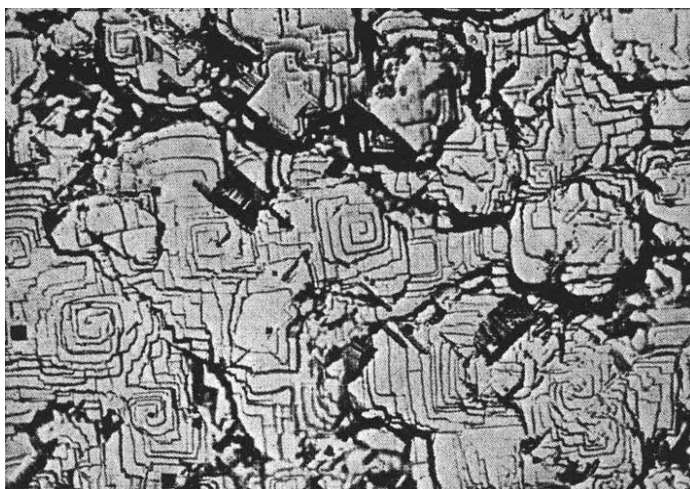
more knowledge is required on how the various groupings of an organic molecule interact with the detailed structure of a metal surface. The authors review the relevant organic chemistry, surface chemistry and metal physics, but it is apparent that it is not yet possible to design organic inhibitors for specific metals in specific environments. The authors provide a most valuable summary of the applications of organic inhibitors in controlling the corrosion of metals in the atmosphere and in aqueous (acid, neutral and alkaline) and non-aqueous solutions.

The review paper on "Anodic Oxidation of Aluminium" (S. Tajima) is detailed and well referenced, but a rather uncritical approach has been adopted and some of the diagrams are far from clear. Nevertheless, the author has brought together the whole spectrum of the anodic behaviour of aluminium, which ranges from the formation of thin dielectric barrier-type films formed in buffer solutions to the thick porous films formed in acid electrolyte solution that dissolve the oxide during its formation.

The review papers that have been selected for volume 1 of this series provide a comprehensive and up to date survey of existing knowledge in four very diverse fields that fall within the confines of "corrosion science and technology", and in view of the increasing importance of environmental deterioration the appearance of this series is timely. The book as a whole strikes an appropriate balance between theory and practice, and can be recommended without reservation to students, research workers and technologists who are concerned with the environmental behaviour of metals and other materials.

L. L. SHREIR

Electrodeposition



Growth spirals of copper deposited from acid copper sulphate. From Fischer, H., *Electrochim. Acta*, 2, 50 (1960); reproduced in the second edition of *Electrodeposition and Corrosion Processes* by J. M. West (Van Nostrand Reinhold, London, 1972; £2.25).

Statistical Mechanics

Concepts in Statistical Mechanics. By Arthur Hobson. Pp. xii+172. (Gordon and Breach: New York, London and Paris, January 1972.) £7.30; \$17.50.

Atoms and Atomic Theory: an Introduction to Statistical Mechanics. By Ralph Baierlein. Pp. xi+486. (W. H. Freeman: San Francisco, October 1971.) \$15.00; £6.60.

THESE two volumes have one thing in common: they both base the discussion of statistical mechanics on Jaynes's view of the subject. This in turn was a development following from Shannon's statement of probability theory and information theory. There may be something to be said for this approach—which to some extent is nothing but a formalization of the point of view that statistical mechanics is the study of very incompletely specified systems—a point of view which is clear from the Ehrenfests' masterly account of statistical mechanics in the *Enzyklopädie der Mathematischen Wissenschaften* (1911) and which was most forcefully advanced by Tolman.

The book by Hobson is a pleasant account of the concepts and mathematical structure of statistical mechanics, well written and well thought out. One wonders, however, why this material is presented in book form. There is very little new material in the book and it certainly is not a textbook, so the reason for issuing it as a hard-cover volume is completely obscure. I feel that the author would have been better advised to have submitted it for publication in a review journal, where it surely would reach a larger public.

The book by Baierlein is a textbook and the author has included a useful collection of problems at the end of each chapter. The main body of the book is straightforward conventional statistical mechanics, and one wonders why the title, blurb, and introduction stress the "alternative" approach. Moreover, if one stresses the information theory approach in a textbook, one should at least give an adequate description of other ways of introducing exactly the same theoretical framework for solving problems in the theory of complicated systems—and this is one of the strong points of Hobson's book.

The ground covered in Baierlein's book—after three chapters on probability theory—is also conventional; topics covered include equilibrium, paramagnetism, the classical limit, partition functions, perfect and imperfect gases, systems of identical particles (where, as so often in textbooks on statistical mechanics, the discussion of the connexion between spin and statis-

tics is sloppy and incomplete), the free electron model of metals, Bose-Einstein condensation, black-body radiation, magnetic cooling, negative temperatures, and the density matrix. The discussion of these topics is on the whole thorough and sufficiently detailed for students. The book is a worthwhile addition to the already very large choice of texts on statistical mechanics. D. TER HAAR

Geochemical Indicators

Geochemical Facies Analysis. By Ernst Werner. (Methods in Geochemistry and Geophysics, Vol. 11.) Pp. vi+152. (Elsevier: Amsterdam, London and New York, 1970.) £3.50.

THIS book is an extended review which gives primary consideration to assessing the value and potential of those chemical elements and compounds which have already been widely accepted as geochemical indicators. The review draws on a substantial number of geochemical studies of facies which have been carried out during the past twenty-five years and, with its list of approximately 300 recent references, it should form a useful and relatively inexpensive source book for the student of sedimentary geochemistry.

The earlier part of the review is devoted chiefly to a discussion of factors which can affect the results of geochemical facies analysis; not only sampling, preparative and analytical methods, but also the broader influences on the composition of the sediment of climate, tectonism, diagenesis and metamorphism, sediment source and rate of sedimentation. Professor Werner finds that few, if any, of the definitions of facies so far proposed are satisfactory for geochemical purposes, either because they ignore geochemical data altogether, or because they are insufficiently refined. Instead, a special facies unit is proposed, the "hydrofacies", which refers to the influence of the physical and chemical composition of water on the sediments. The results of geochemical facies analysis are considered within the context of the three subdivisions of the hydrofacies that can be recognized—the salinity, oxygen and temperature facies. The reader is left in no doubt about the generally unsatisfactory nature and limited application of geochemical indicators within the salinity facies and it is acknowledged that the temperature facies is the most difficult subdivision of the hydrofacies to recognize. Studies which demonstrate the original oxygen distribution of the sediments are at present felt to be potentially the most useful and reliable, although, in the future, isotopes, and particularly organo-metallic complexes, are suggested as being

the most promising environmental indicators.

Besides drawing together and analysing, albeit briefly, the results of a large number of investigations, an important contribution of the review is that it serves as a warning that no single chemical element or compound can be regarded as an infallible indicator of a specific depositional environment. The value of geochemistry is acknowledged, but more precise evaluations of the nature of environments are likely when geochemical data are combined with facies evidence from other geological sources.

Although there are some quaint usages of English in the review, generally the text reads easily. The two short chapters on non-chemical methods of facies analysis and the practical use and future importance of geochemical facies analysis could both have been profitably extended, particularly the former, but the lack of development of these sections does not detract significantly from a useful addition to this series on geochemical and geophysical methods.

D. G. MURCHISON

Stellar Elements

Nuclear Reactions in Stellar Surfaces and their Relations with Stellar Evolution. By Hubert Reeves. Pp. x+87. (Gordon and Breach: New York, London and Paris, August 1971.) £4, \$9.60 cloth; £1.95, \$4.75 paperback.

THE existence of the light elements deuterium, lithium, beryllium and boron presents a special challenge to theories of nucleosynthesis in stars because the low stability of their nuclei makes one expect them to be largely destroyed under typical conditions in stellar interiors. The relative abundance of these elements in cosmic rays, however, provides a clue as to how they were probably formed, namely by spallation reactions between energetic protons and heavier nuclei in a rarefied medium (though other possibilities exist for D and ⁷Li). This general idea has received powerful support from a brilliant series of experimental and theoretical studies at the Institut de Physique Nucléaire, Orsay, in which the relevant cross-sections were measured and found to predict to a good approximation several of the elemental and isotopic abundance ratios of light elements observed in the solar system and a number of stars. Where the reactions that produced most of these elements actually occurred is another question. Originally the most likely location was thought to be in surface layers of the stars themselves, during flare activity such as is observed in the youthful T Tauri phase of evolution, but this hypothesis involves grave

energetic difficulties. More recently, cosmic rays have been shown to be a plausible source, feeding the interstellar medium with light elements that the stars simply inherit from it (along with other elements) when they are born, but there may also be a thermonuclear contribution to ^7Li .

This little book, based on a series of lectures by a leading member of the Orsay team, gives a clear and concise introduction to the physical and astrophysical considerations involved. The observations are very well summarized and possible explanations put forward, while at the same time making clear what is reasonably well established, what is still somewhat doubtful and what is frankly speculative. The second category contains ideas as to where spallation most probably occurred and the author frankly confesses that he changed his mind on this point during production of the book, although the stellar surface reactions referred to in the title remain relevant as a cause of depletion of light elements if not of their creation.

The book can be warmly recommended as an authoritative and readable introduction to an interesting and rapidly moving field.

BERNARD PAGEL

Spectroscopy with Lasers

Laser Raman Spectroscopy. By Marvin C. Tobin. (Vol. 35.) Pp. xi+171. (Wiley Interscience: New York and London, 1971.) £6.50.

Spectroscopy with Lasers. By W. Demtröder. (Topics in Current Chemistry, Band 17.) Pp. iii+95. (Springer Verlag: Berlin and New York, 1971.) 28DM; \$7.70.

LASERS are now relatively familiar tools in many areas of pure and applied science. In spectroscopy they have made possible considerable advances in the practical application of certain well-established spectroscopic techniques, particularly those associated with light scattering; they have also led to the discovery of new forms of spectroscopy. Between them these two books treat many of the applications of lasers to spectroscopy.

Using gas lasers for excitation and more refined optical and detection systems, it is now possible to obtain the Raman spectrum of almost any material at least as readily as its infrared spectrum. Since the information derived from Raman spectroscopy is complementary to that obtained from infrared spectroscopy, more and more chemists will now be wanting to use Raman spectroscopy as well as infrared spectroscopy as a routine spectroscopic tool. Professor Tobin's book could prove helpful to such chemists.

Most of the first chapter on the

nature of the Raman effect is in fact an account of group theory and normal vibrations; only ten pages are devoted to the theory of the Raman effect which will be unfamiliar to many chemists. A working knowledge of the theory is important particularly when interpreting depolarization ratios and orientation effects in single crystals, and the very condensed account given here is hardly sufficient. A statement on page 12 is misleading. It is stated that for illumination with plane polarized light, ρ may vary from 0 for large α to 0.75 for large β . In fact ρ is zero if β is zero (irrespective of the value of α) and 0.75 if α is zero (irrespective of the value of β).

The second chapter on experimental methods should prove more useful. Two important topics, the maximization of the scattered light flux reaching the detector from a sample illuminated by a laser source and the factors affecting signal-to-noise ratio, are quite fully treated. Many other practical aspects of Raman spectroscopy are also dealt with, including photomultipliers, methods of signal processing, commercially available Raman spectrometers, determination of Raman band characteristics (intensities, contours and depolarization ratios), special procedures for the study of single crystals and gases, sample handling and laser sources. Some of these topics are unfortunately given very little space: for example, just one page is devoted to lasers and ten lines to lock-in amplification. The account of sample handling techniques is also far from complete.

The third chapter on interpretation of Raman spectra is mainly devoted to group frequencies. There are useful comparative discussions in which group frequencies in infrared and Raman spectra are compared for many classes of compounds. Unfortunately the information on Raman group frequencies is far less complete than for infrared group frequencies. Indeed Tobin's discussion has had to be based on Raman frequencies obtained with mercury arc excitation and previously listed by Brandmüller and Moser and by Szymanski.

The book ends with an attempt to cover in twelve pages recent applications of Raman spectroscopy to polymers, biological materials, minerals and inorganic glasses, inorganic single crystals, ionic melts and complex ions in solution, water, simple covalent compounds and organic chemistry! In addition to a list of papers specifically referred to in the text, there is a bibliography of publications on Raman spectroscopy for the years 1968–69.

This book is written in a pleasant conversational style which will make it attractive to chemists seeking a practical introduction to the subject.

Professor Demtröder's seventy-nine page essay entitled *Spectroscopy with Lasers* is a valuable introductory survey of the many and varied applications of lasers in spectroscopy. The topics covered include: absorption and fluorescence spectroscopy; photochemistry; Raman spectroscopy and light scattering; plasma spectroscopy; studies of measurements of lifetimes of excited states and collision processes; microanalysis; saturation spectroscopy and spectroscopy of laser media. There is a useful introductory chapter dealing with the characteristic features of lasers as spectroscopic light sources and a good bibliography of more than 400 references.

Of course, the various topics are not treated in depth but the value of this little book lies in the very wide range of applications which is dealt with. Chemists in particular will find it stimulating to read this essay.

D. A. LONG

Crystal Structure

The Crystalline State. By Peter Gay. Pp. ix+348. (Oliver and Boyd: Edinburgh, January 1972.) £5.

THIS book has been designed for undergraduates studying a number of different sciences including crystallography, chemistry, physics, mineralogy and metallurgy. The mathematics required is no more than a first year student may be expected to have. The development of the subject differs from corresponding earlier treatments in that the ideas of symmetry are dealt with in terms of the internal structure of crystals just as much as in terms of their external faces. The study of two-dimensional patterns is followed by that of point groups, morphological problems and space groups. There are three chapters on X-ray diffraction from single crystals and from powders. A brief sketch of the physical properties of crystals is followed by a chapter on dislocations. There are four appendices on projections, crystallographic calculations, growth and twinning.

The text is clearly written and bears witness to the long experience of teaching the subject which the author has had. Each chapter is followed by many examples to be worked out and answers are given. The excellence of the text is, unfortunately, marred by the errors in the text-figures: in several, for example, edges of faces which should be parallel to one another are not. Other instances include the failure of a small circle about a point on the primitive circle to cut it at right-angles; incorrect indexing in Fig. 5.5(b), giving two positions for the point 001, and arcs which should be circular and are not. Fig. 9.2 has an S-shaped line through Laue spots which

the legend says represents a zone of reflecting planes, whereas in fact it represents two halves of two distinct zones; Fig. A.1 shows points W and X within the primitive circle whereas they must be outside because the tangent plane cannot cut the equator inside the primitive circle; Fig. A.6 shows the point Q on the near side of the projection sphere but the gnomonic point corresponds to it, being on the far side.

W. A. WOOSTER

Practical Chromatography

Preparative Gas Chromatography. Edited by Albert Zlatkis and Victor Pretorius. Pp. viii+402. (Wiley: New York and London, 1971.) £8.50.

WHEREAS analytical gas chromatography has been one of the most successful techniques of the last two decades, preparative gas chromatography has never lived up to its initial expectations. There remains, however, a steady small application for preparative gas chromatography for small quantities (usually 100 mg–100 g) of pure materials not easily purifiable in other ways and this is ably described in this collection of essays. Being on a severely practical topic, the book will appeal to severely practical readers, that is, those who have a specific problem which might be solved by the technique, or those who have a commercial apparatus which they wish to use to best advantage, or those who plan to make their own apparatus. The book caters for all these three categories of reader.

There are three essays on specific applications; to general organic chemistry (Verzele), to flavour research (Merritt), and to biomedical applications (VandenHeuvel and Kuron). The latter two essays are well documented and one has little doubt that they summarize all the significant applications in their fields up to 1968 or 1969.

The book caters particularly well for the apparatus builders. Inlet systems and collection systems, and all the problems associated with such things as introducing samples to columns in compact zones, and preventing fog formation while condensing products from the gas stream, are dealt with very fully by Hupe. Other chapters on the design technology are by Busch on programming repetitive runs, and by Kaiser on temperature and flow programming. The technique of making preparative columns, a topic which will be required by most users, is well covered in a chapter by Pecsar where, though the style is often reminiscent of a manufacturer's glossy, the context is as informative as the state of the art

allows. It must be admitted that a novice at the art might find himself confused at the multiplicity of imperfect techniques described.

The theory of preparative gas chromatography is covered by Pretorius and de Clerk, who try hard to make the best of a bad job. In preparative gas chromatography the aim is to produce as much product as possible, as pure as possible, as quickly as possible, and the theory is studied in the hope that it will help one predict the parameters required for this aim. The trouble is that there are so many parameters that optimization procedures are rarely unambiguous. There is a final chapter (Barker) on the appealing but technically difficult device whereby the column is made circular, and by rotating the column with respect to the inlet and outlet ports one can conduct continuous chromatography.

The book is well edited with little overlap between chapters. The balance of the book in general is as good as could be produced on a not altogether satisfactory topic.

A. B. LITTLEWOOD

The Caledonides

Geology of the East Greenland Caledonides. By John Haller. Pp. xxiii+413. (Wiley: London and New York, December 1971.) £20.

THE appearance of this magnificent volume in the series on the regional geology of the world, edited by Professor de Sitter, marks an important stage in the understanding of the East Greenland Caledonides and in the achievements of Professor John Haller alike.

The brief, and therefore partial, view of Greenland exploration in the preface is of special interest; the contribution of Laue Koch is clarified and the achievements of his remarkable series of expeditions between 1916 and 1958 are given justifiable emphasis in the context of this book and their great intrinsic value to the geological reconnaissance of Greenland.

After a brief chapter on physiography a comprehensive but succinct history of the geological exploration of East Greenland is presented, although the coverage of the research of the last decade on the northernmost parts of the island is relatively slim.

The analytical critique of the style of tectonism of the Caledonian fold belt and discussion of basic problems and controversies are particularly valuable and should serve as a framework for the further research which must be needed, particularly in the light of current plate tectonics theory and the question of the sundering of the

Caledonides of East Greenland, Spitsbergen, Scandinavia and the British Isles and a Lower Palaeozoic "Proto-Atlantic". In this chapter remarks on the author's techniques of interpretation of aerial photographs coupled with his personal observations from the air indicate significant factors in his methods; these were found to be more precise than "normal field work" on the excellently exposed mountain landscapes. Landings were made at key localities in the East Greenland fold belt (75°–82° N) but it is clear that more ground control was used in the southern part (70°–75° N), which was studied in more detail in earlier years. In the far north-east (79°–82° N) it seems fair to say that Haller has relied to a great extent on the other workers for his ground control and has personally made only few landings at key localities; lack of finance can be blamed and the unexpected termination of the series of Koch's Danish expeditions in 1958. More published details of Haller's field-work would provide a clearer basis for evaluation of the need for future research.

The "Pre-Caledonian Succession" (chapter 3) includes a section on Precambrian evolution with valuable summaries on the basement complexes of north, west and east Greenland. The section on the Thule Group and Koch's "Thule Formation" is a useful summary of early work but is regrettably incomplete: no discussion is presented of recent papers, from 1966 onwards, by Kerr, Christie and others in arctic Canada; by Jepsen, Dawes and Soper in northern Greenland or of recent discussions by Dawes and Cowie. Haller's main topic is the East Greenland Caledonides and this is not greatly affected by controversies regarding the value of his stratigraphical framework in north and north-east Greenland (Hagen Fjord Group, Thule Group and their ages) and the Carolinian Fold Belt.

The section on the Lower Palaeozoic strata is well presented for both northern and eastern Greenland with clearly designed and draughted maps, sections and tables coupled with excellent photographs.

The heart of the book is undoubtedly chapter 4 on the Caledonian Orogeny, which is consummately done. A slight criticism is that in some maps the topographic outlines are a little fine (for example Fig. 58 on page 152) but the draughting and printing are sharp and clean and all the details can be made out with careful examination.

Concluding chapters on Post-Caledonian and Cainozoic events are followed by a valuable selected bibliography which is comprehensive but has few entries in the late 1960s (for various reasons); a new generation of

papers comes forward now. The indexes (author and subject), which fill thirty-five pages, are commendably detailed.

This is a book of outstanding merit and a milestone on the road which Lauge Koch and others pursued and still follow: "to know and see always a little more of Greenland's geology".

J. W. COWIE

Glaciation

Glacier Ice. By Austin Post and Edward R. LaChapelle. Pp. 110 (130 photographs). (University of Washington: Seattle and London, 1971.) \$20.

THIS book captures both the magnificent alpine scenery of glacier covered mountains and the fascination of nature, manifest sometimes in well ordered glacier patterns and at others in the chaotic jumble of an ice fall or the jagged penitents of a high snowfield in Chile. The regular bowed curves of the ogives, the long straight medial moraines typical of most glaciers, or the whorled moraines of a surging glacier all call out for an explanation.

Such explanations are provided by two experienced glaciologists with a special knowledge of the mountains and glaciers of the north-west of North America. Austin Post is well known for his use of aerial photography as an aid to glaciological investigations, while Edward LaChapelle of the University of Washington, Seattle, has made many contributions to the study of ice crystals, avalanches and glacier flow. Together they have produced a book of character.

Although *Glacier Ice* would grace any coffee table with its superb black-and-white photographs in a large landscape format, it is primarily a reference book of glacier photographs. These are arranged in sections to illustrate various aspects of glaciology ranging from glacier formation, mass balance and surface features, to the flow of glaciers, surging glaciers, moraine formation, glacial landscapes and differences in appearance of temperate, sub-polar and polar glaciers.

Accompanying the photographic material is an authoritative text which, without use of mathematics or complex physical terminology, describes the phenomena in the photographs. A useful glossary of glacier terms is included.

One can only make minor criticisms of this production. Perhaps more use of diagrams (there is only one) to explain phenomena would have helped the layman. The brief section on the Antarctic does not succeed in giving the reader a feel for the vast scale of the phenomena, especially in relation to



the Ross Ice Shelf. The authors are primarily "mountain" glaciologists, however, and more than ninety per cent of their photographs deal with phenomena in mountain regions. They have done an excellent job which can be recommended both as an aid to teaching and especially to those who have been fascinated by seeing a few glaciers in nature and wish to learn more for their own satisfaction.

G. DE Q. ROBIN

Photochemistry

Annual Survey of Photochemistry. By Nicholas J. Turro *et al.* Vol. 3. Pp. x+382. (Wiley: New York and London, August 1971.) £9.50.

Photochemistry. Vol. 2. Senior reporter, D. Bryce-Smith. Pp. xvii+817. (The Chemical Society: London, May 1971.) £12.

THESE are continuing volumes of two annual series which survey one year's literature in the popular area of photochemistry. The volumes do not quite overlap; the survey by Turro *et al.* covers the 1969 literature, while the Chemical Society (CS) volume covers the period July 1969–June 1970.

The Turro book is in three parts. The first part, which is on organic photochemistry and contains 1,071 references, provides requisite coverage but is in many ways unsatisfactory. There is usually only a brief summary comment on each paper, which gives little indication of the detailed content of the paper (for example, reaction conditions, quantum yields, rate constants, basis for mechanistic conclusions and so on) except in rare instances where actual data are presented. There is an unfortunately large number of typographical errors, mostly in structures. It is useful as a quick superficial view of what has been done, and the interested reader can find the details in the references. I find Part II by Leonard and Hammond, which deals with photophysical processes of organic compounds (1,052

references), more satisfactory, for the subject matter is grouped into several very useful tables (for example, polymolecular effects in absorption spectroscopy, radiative decay of excited states, and so on), continuing the format established in the previous volume of the series. There is also a general discussion of progress in the area of photophysical processes, with valuable commentary and assessment of key contributions reported during the year. The short third part on coordination complexes is in the form of a discussion with editorial commentary.

The CS volume is subdivided into smaller sections than the Turro volume and provides a detailed discussion of the contents of the literature for the period covered, including experimental design, primary data including many tables, graphs and spectra, kinetic schemes, rate equations, and the main arguments presented in the original paper, with critical commentary. Thus the CS survey is far more comprehensive than the Turro survey, and includes sufficient background on most topics to provide a context for discussion of recent papers, whereas this is almost never the case in the Turro volume. A reader relatively non-conversant with the background literature in photochemistry could benefit from the CS survey, but would be generally at sea in the Turro survey, except perhaps in Part II. The introduction and general review of the year in the CS volume by Bryce-Smith, the senior reporter, is interesting as an overview of progress in photochemistry by an acknowledged expert. Both volumes include author indices.

I personally refer to the CS volumes more frequently than to the Turro surveys, although both are useful. Because they are expensive, it is doubtful whether most photochemists will want to purchase both volumes, in which case I would recommend the CS survey. Professional libraries in laboratories and institutions with activity in photochemistry should preferably have both volumes in their collection.

DAVID I. SCHUSTER

High Magnetic Fields

Pulsed High Magnetic Fields: Physical Effects and Generation Methods Concerning Pulsed Fields up to the Mega-oersted Level. By Heinz Knoepfel. Pp. xxii+372. (North-Holland: Amsterdam and London, 1970.) £8.40.

THE production of intense steady magnetic fields is, and always has been, a costly business. It is therefore not surprising that the pattern which has

emerged during the past fifteen years or so has been that of a small number of centrally-situated laboratories. Most scientifically advanced countries have their own nationally-funded magnet laboratories, the exception being Great Britain which has neither laboratory nor plans for one. Typically, a laboratory of this kind has available about 10 MW of d.c. power and cooling facilities; it offers steady magnetic fields of up to about 15 T to those prepared to perform their experiments away from home. Many laboratories are now seeking to increase the field strength which they can reach and it seems that the practical limit, which at present is probably about 30 T, is within sight of being reached. Beyond this figure problems of stress and heat transfer become so acute that the only practical solution is to use pulse techniques.

The subject of pulsed magnetic field systems has a distinguished ancestry with names such as Fabry, Perot, Cockroft and Kapitza figuring largely in its development. Within the past fifteen years or so the generation of high intensity pulsed fields has been the subject of intense activity in the laboratories of many atomic energy authorities largely because of the potential use of magnetic fields in the containment of plasma-fusion devices. An immense body of experimental information and theoretical understanding has been built up, much of it contained in internal reports and rarely collected together in a systematic way.

One of the most distinguished of these laboratories is at Frascati, Italy. It is run under the auspices of the European Atomic Energy Commission and the Comitato Nazionale Energia Nucleare. Its high field research group is led by the author of this book and into it he has distilled eight years' experience with pulsed magnetic field systems. Although written from the standpoint of an experimentalist the theory is developed *ab initio* and in full. The basic theory begins with the calculation of quasi-stationary magnetic fields and is then extended to cover magnetic diffusion theory. There follow sections on heating and stress effects in current-carrying conductors. Energy storage systems, the design of coils, flux compression systems including the use of implosive techniques, are all treated in detail. The final chapters deal with measurement techniques and the behaviour of conductors at high energy densities. There is an exhaustive list of references.

This is a treatise and is in every sense worthy of the title. It will undoubtedly become the "bible" for all those engaged in research involving pulsed magnetic fields. Would that every author delayed the writing of his book until he had as much to say as Dr Knoepfel.

E. W. LEE

Geochemical Method

Modern Methods of Geochemical Analysis. Edited by Richard E. Wannerdi and Ernst A. Uken. Pp. xviii + 397. (Plenum: New York and London, 1971.) \$25.20.

THIS text represents yet another attempt to summarize the large variety of analytical techniques, both chemical and mineralogical, used in geochemistry.

The introductory section opens with a definition of geochemistry and an outline of the broad fields of application of geochemical analysis. Surely the science is past the stage of requiring a definition, nor is there need to justify geochemical analysis as an investigative method in the earth sciences. A useful, though elementary, chapter on statistics follows the introduction. Apart from defining statistical parameters, this contains a limited number of examples of the use of statistics with respect to the need or otherwise of replication in analysis. Succeeding chapters, written by authorities in the particular fields, deal with various methods of analysis. In sequence they include: "Chemical Analysis and Sample Preparation," "Ion-Exchange Chromatography," "Colorimetry," "Infrared Spectrometry," "Optical Emission Spectroscopy," "Atomic Absorption," "X-ray Techniques," "Radiometric Techniques," "Nuclear Activation Analysis" and "Mass Spectrometry." The criteria underlying the choice of methods must remain a mystery. For example, why is ion-exchange chromatography included but not paper, thin-layer or gas chromatography or, in connexion with mass spectrometry, why is age-determination mass spectrometry excluded?

The chapters basically cover similar aspects including historical development, basic principles, sample preparation and presentation, apparatus, techniques, sensitivity, precision and accuracy, geochemical applications and recent and future developments. Because of the wide variety of authors, however, the description of each method varies very widely within the above framework. Some authors stress basic principles while others, much more usefully in the context of this book, stress the applications and developing utility of the technique, illustrating their discourse with well chosen examples. Clearly principles, which have changed but little over a number of years, could be fully covered by reference to authoritative and definitive texts. This approach is adopted by many of the authors who specifically recommend further reading, particularly on this aspect. The opportunity for further reading is certainly present, as each chapter is endowed with an extensive, up to date bibliography. In most

chapters the advantages and disadvantages of particular techniques are evaluated, and in some instances the information allows an assessment of the relative merits, and cost, of complementary or alternative methods. One useful facet introduced by many authors deals with probable future developments. These techniques, still in their experimental phase, may ultimately become important analytical methods.

As a general reference work this book should be a useful addition to most libraries; priced at \$25.20, however, it is difficult to recommend for individual purchase. D. M. HIRST

Organic Electrochemistry

Electrochemical Reactions in Non-aqueous Systems. By Charles K. Mann and Karen K. Barnes. (Monographs in Electroanalytical Chemistry and Electrochemistry.) Pp viii + 560. (Dekker: New York, October 1970.) \$34.50; £16.40.

IN the past decade, there has developed a clear need for an authoritative and well balanced text on organic electrochemistry to provide a common base for both the organic chemist and the electrochemist working in this area. Unfortunately, neither this book nor the other recent texts by organically inclined authors comes close to filling the gap. Indeed, this volume is concerned essentially with the electrochemistry of organic compounds in non-aqueous media; the single final chapter on inorganic compounds provides insufficient justification for a wider claim. An introductory chapter on the interpretation of electrochemical measurements is included to help readers who have insufficient backgrounds in electrochemistry. It is too short to be of much value in this respect, and its main utility is in giving data on the behaviour of background electrolytes and reference electrodes in a number of solvent systems.

The authors state that the book is based on a literature search covering the period to the end of 1968. The result is a systematic but rather uncritical review of this literature together with a large and useful compilation of the electrochemical reaction potentials of many compounds. There are chapters dealing separately with particular classes of compounds and no further attempt is made to show the underlying unity of the subject nor to discuss the many urgent and unanswered questions in the field. One looks in vain for the authoritative touch, the panoramic view that might delineate the skeletal structure on which the great mass of data can be hung, so that the basic chemistry is emphasized. Thus the reader is neither stimulated nor guided but merely in-

formed. It must be conceded that the information provided will be of great use to research workers in the field, and the publishers' claim that the book serves as a reference and guide to the literature is well justified. This function is, however, vulnerable in view of the rapid development of the subject since 1968 and in many respects the book is already outdated. Thus one questions the whole concept of producing a review of this type in book form rather than as a regularly updated series in the review literature. A glance at the price strongly reinforces this view.

A. BEWICK

Elements in Meteorites

Handbook of Elemental Abundances in Meteorites. Edited by Brian Mason. (Series on Extraterrestrial Chemistry.) Pp. ix+555. (Gordon and Breach: New York and London, December 1971.) £14.60 cloth; £5.80 paper.

THE little sample of extraterrestrial material furnished by the meteorites provides the basis for many of our ideas about the early state of the solar system, the origin of the planets and the nature of the Earth's interior. It deserves careful study; and since 1953, and especially in the past decade, an immense amount of detailed chemical analysis, by ever more refined methods, has been carried out on meteorites. This book, sponsored by the International Association of Geochemistry and Cosmochemistry, is an attempt to make the results more accessible.

The result is really a synopsis rather than a handbook. It does not set out to compile the basic data on the chemical constituents of individual meteorites, but furnishes instead a statistical summary of the abundances of the elements in meteorites grouped into classes. Although there are many abundance values appertaining to named meteorites in the book (thirty-nine of the sixty-five chapters on individual elements give some such data), this is not the main aim.

It is arguable that, in the present state of knowledge, a true handbook of critically evaluated data would have been more useful than a synopsis. Since the synoptic form was chosen, however, clearly it was desirable to decide firmly on a single classificatory scheme and a uniform set of symbols to be used throughout. This has not been done with a sufficiently firm hand, and the resulting anarchy over terms and notation mars the book. It would have been valuable, too, to have added an agreed classificatory symbol to each of the meteorites in the index of meteorites at the end. It is interesting that most of the twenty-one con-

tributors are attracted to the Van Schmus-Wood classification of chondrites, but the editor conspicuously is not.

We begin with a nineteen-page introduction mainly devoted to classification. Unfortunately this does not cover some of the terms and symbols used later in the book. Then follows a chapter on each chemical element arranged in periodic order (the inert gases and rare earths being treated as groups, and Zr and Hf together). In each four to twelve-page chapter (but "Inert Gases" gets 38 pages, K 21 pages and U 20 pages) there is usually a table giving the range and mean amount of the element, and the number of determinations, in each class of meteorites. Often there will be a diagram giving the distribution of determined values by classes. If you are lucky there will be some values for named meteorites and data on separated minerals. The text consists of notes on the tables, commentary on the reliability of the analytical determinations (very professional) and some remarks on the fractionation of the element with respect to others. References, gathered at the end of each chapter, bring one well up to date, being mostly to work published in the 'sixties.

As a source-book the sphere of usefulness of this volume is difficult to assess. It will be valuable in the teaching of general geochemistry and cosmochemistry, but for this purpose it might well have been briefer. For the research worker analysing meteorites and the theoretician thinking about them, its value may depend on which element is of immediate interest. For some he may find tabulated most or all of what is known: for others he will wish to know more about individual meteorites, and will have to turn back to the original literature (to which the *Handbook* is an excellent guide).

M. H. BATTEY

Glass Structure

Structure and Crystallization of Glasses. By Werner Vogel. Pp. 246. (Pergamon: Oxford and New York, October 1971.) £7.

THIS volume is a translation of the original German edition published in 1965. It is an account of the experimental work on liquid-liquid separation and nucleation and crystal growth in glasses carried out by the author and his colleagues in the laboratories of VEB Jena Glass Works of Schott and Gen. The book, which is beautifully produced in large format 8 by 11.5 inches, contains a large number of reproductions of optical and electron micrographs. The publishers can hardly have expected the

book to be treated as a textbook but its appearance in English translation will be welcomed in any laboratory where the topics discussed in the book are being investigated or where a project in this field is being considered. It is, however, a difficult book to review, for although it contains clear accounts of some excellent experimental work it could be criticized because it lacks quantitative data. There is, for example, no mention of the measurement of immiscibility temperatures and the heat treatments reported do not reveal any systematic selection.

The book has now been in print for more than six years, however, and its value lies in the record it provides of some excellent pioneering work. This value is enhanced by the detailed descriptions given of the preparation of glasses and the procedures for electron microscopy.

The first sixty pages describe the preparation and study of beryllium fluoride glasses which Vogel regards as "prototypes" of silicate glasses. This is probably the most complete account available of the study of these glasses. BeF_2 has the structure of β -cristobalite and BeF_2 glass has the same tetrahedral-type structure as SiO_2 with the Be-F distance 1.6 Å compared with 1.62 Å for Si-O. Vogel looks on BeF_2 and alkali beryllium fluoride glass systems as "weakened" counterparts of the corresponding silicate systems; certainly the photographs reveal remarkable similarities in the appearance of liquid-liquid separation and precipitation of crystals.

Other sections of the book describe studies of $\text{Li}_2\text{O-SiO}_2$, $\text{Na}_2\text{O-B}_2\text{O}_3\text{-SiO}_2$ glass-forming systems as well as commercial compositions used for the preparation of glass-ceramic materials.

Vogel concludes, "The results described . . . confirm that the processes of segregation play a primary part in the conversion of a Pyroceram glass into a ceramic product". By segregation here he means liquid-liquid immiscibility.

In one or two places the translation could be improved but no difficulty arises and the volume will be read with interest by all concerned with these topics.

R. W. DOUGLAS

Light on Molecules

The Theory of Optical Activity. By Dennis J. Caldwell and Henry Eyring. Pp. viii+244. (Wiley Interscience: New York and London, November 1971.) £7.

THE physical basis of optical activity is very simple to understand and to formulate quantum mechanically, but the application of the theory to the calcula-

tion of the activity of molecules, real or abstract, is remarkably difficult. This difficulty stems from the sensitivity of the equations to the form of the molecular wave function: because optical activity depends on both an electric and a magnetic dipole transition it is necessary to calculate two transition moments, and their relative phase is important. The discussion of these complexities is the substance of this volume, and although a number of reviews have appeared in diverse publications this is the first extensive, coherent account of the theories of optical activity since Lowry wrote his excellent book while quantum mechanics was too young to penetrate its mysteries.

And what a fierce theoretical problem it is! The first part of the book discusses in detail the classical theory of optical activity which is based on the coupled oscillator approach where an electron oscillates linearly and is coupled to another oscillating linearly or in a circle, or an electron oscillates in a helix. Although inadequate, such an approach is important because the pictures it provides enable one to introduce appropriate approximations into the quantum mechanical exegesis that follows. The bulk of the book derives and then develops the quantum mechanical theory by discussing the general equations for many-electron systems, and then applies it to specific chromophores (the carbonyl group, benzene derivatives, and transition metal complexes). Then a chapter, which is regrettably too short, is devoted to the Faraday effect, and another to a mixed bag of special effects, which include vibronic transitions and excitons. The book concludes with a very brief survey of the methods of interpreting some experimental data.

The book is welcome because it was so badly needed. It is difficult, but lucid, and it will doubtless stimulate a great deal of thought. My only regret is that it is so brief. P. W. ATKINS

Electrochemistry

Physical Methods of Chemistry. Part 2A: Electrochemical Methods. Edited by Arnold Weissberger and Bryant W. Rossiter. Pp. xiv+723. (Wiley: New York and London, November 1971.) £15.

BOTH the cost and the nature of this book make it essentially a work of reference to be sought in the library. The aim is to provide the reader with an appreciation of the techniques, the basic theory and apparatus involved. There would seem to be a number of important fields of investigation for which this information could be

required, such as the general properties of electrolyte solutions covering reference electrodes, the nature of ions in solution, ion association; analytical procedures, electrochemical synthesis; the study of mechanisms in both electrochemical and chemical reactions. It is against these requirements that some assessment of the present volume is advanced.

The earlier chapters deal broadly with electrolyte solutions — matters with which most chemists are more or less familiar. In the first chapter, on oxidation-reduction potentials, confusingly and surprisingly use is made of single electrode potentials; chapter 2 is concerned with pH measurements and ion selective electrodes, titration curves, dissociation constants and similar problems; chapter 3 deals with conductance and chapter 5 with transport number measurement.

There is no account of high temperature and pressure procedures which surely should have a place at this point in a book of this kind. Material presented in these chapters is extensive and accessible but in general they add little to the ease of obtaining similar information from other sources.

Chapter 5, on polarography, would no doubt be mainly consulted for the basis of analytical applications, and the introduction deals admirably with information problems with discussion also of applications to kinetic studies. The author is naturally very conscious of the large and excellent coverage of both these areas in existing books and monographs. Chapter 9, on controlled potential analysis, also deals with studies of reaction mechanisms. The general reader is much less likely to be familiar with the treatment of kinetics by electrochemical methods and this could have been the most valuable part of the book.

An initial, if minor, point must be made; electrochemistry is bedevilled with jargon words for techniques; for example, cyclic voltammetry, chronoamperimetry, chrono-coulometry, chronopotentiometry, even perhaps potentiometry, and as such they make a poor choice for chapter headings.

If chapters 6, 7 and 8 are considered together there is a great deal of common interest in kinetics in each. A real complaint about the book is that there is an arbitrary editorial subdivision of the presented material which blurs the unity of interest. It also gives unjustified weighting of one technique against the other; for example, 10 pages on the use of rotating electrodes compared with 112 on linear potential sweep amperimetry and the appearance of the latter in two chapters; an inadequate treatment of small scale perturbation methods, particularly sine wave perturbations; the unnecessary

repetition of theoretical treatments of the same general problems of chemical, electrochemical and diffusion control in these kinetically based chapters.

The sum total of these chapters is thus unhelpful in the sense that the necessary analysis for effective presentation has not been worked out at editorial level. This is not a reflexion on the competence of the separate chapters but it does mean that the use of the presented material would require, for interpretation, an investigator with skills comparable to those of the authors themselves. Thus the editorial treatment of chapters 5 to 9 has not helped in any way the expressed intention of the series.

Altogether this is not a happy book and most certainly it could have benefited very greatly by using the same authors with more appropriate terms of reference. As an electrochemist I would still consider it a useful volume to be found on the library shelves, and the earlier chapters 1 to 5 could be readily consulted by an experienced physical chemist. It would be difficult, however, to imagine effective reference to the later chapters being made by anyone, unskilled in electrochemistry, wishing for a balanced insight into modern electrochemical techniques. That these deal often with fast reactions, and that some of the methods were those laying the foundation of modern studies of fast reactions by perturbation methods, would never emerge.

There are a number of other fields of electrochemical experimentation omitted but coverage of these may be contemplated in future volumes.

H. THIRSK

Undergraduate Topology

Topological Geometry. By I. R. Porteous. Pp. vii+457. (Van Nostrand Reinhold: New York, London, Toronto and Melbourne, 1971.) £3.00 paper; £6.00 cloth.

THIS book, which is intended for undergraduates in their final year, consists of a basic course in affine approximation. It aims at applying linear algebra to analysis from a geometric point of view, in a way parallel to that of J. Dieudonné in *Foundations of Modern Analysis*. The book may be conveniently divided into three parts, each of which could be used as the basis for a term's lecture course.

The first eight chapters give a treatment of many of the fundamental notions from a standard course in algebra: sets and maps; the natural numbers; groups, rings and fields; the fields of rational, real and complex numbers; linear spaces and affine spaces. The geometric viewpoint shows itself in

the treatment of affine spaces and the discussion of hyperplanes, orientation and Grassmannians.

The second part, comprising chapters 9–14, is an introduction to geometric algebra, beginning with Euclidean spaces and bilinear forms. Then follows a discussion of the geometry of the quaternions, correlations, and correlated spaces, which culminates in the definitions of the classical Lie groups and the interpretation of Grassmannians and quadrics as homogeneous spaces. Finally, there is a good exposition of Clifford algebras and Cayley algebra.

The last six chapters present an introduction to differential calculus in Banach spaces, leading up to the definition of differentiable manifolds. They begin with the elements of normed linear spaces, topological spaces, topological groups and topological manifolds. Then differentiable maps are defined for Banach spaces, their properties are studied, and the inverse and implicit function theorems are proved. This is then used as the basis for a brief introduction to smooth manifolds, tangent bundles, embedding theorems, Lie groups and Lie algebras.

The book is well set out with a good index and a list of symbols used, (some of which, unfortunately, are non-standard). It has plenty of examples and there is a wealth of problems at the end of each chapter. By way of criticism, however, it should be said that the first section is more than amply covered in existing texts, and it might have been better to have assumed a knowledge of the material covered there in order to produce a book which, in paperback at least, might have had a price much more attractive to the undergraduate. It is also debatable whether the material covered in the second section is wholly suitable for an undergraduate course. Nevertheless, the book should prove to be very profitable prerequisite reading for an MSc course in topology or geometry.

L. M. WOODWARD

Markov Chains

Discrete Markov Chains. By V. I. Romanovsky. Translated from the Russian by E. Seneta. Pp. xi+408. (Wolters-Noordhoff: Groningen, The Netherlands, 1970.) Hfl.66.85; \$18.65.

VSEVOLOD IVANOVICH ROMANOVSKY (1879–1954) was one of the founding fathers of mathematical statistics in the Soviet Union. Born at Alma Ata, he studied at St Petersburg University, presumably under Markov, but returned to Central Asia, where he spent most of his working life, and built up a small

but active school in probability and mathematical statistics. During the 20s and 30s he wrote a large number of articles on a variety of topics in mathematical statistics, including several textbooks. Markov chains formed a major interest in the later part of his life, and the present book, the first of his books to be translated into English, contains a systematic account of his work on this topic.

In essence, it is a treatise on the application of classical matrix theory to a variety of problems in finite Markov chains. Much of the basic material overlaps with the monograph by Fréchet¹, or is subsumed by later work such as the texts by Feller² or Chung³. Of the more specialized material a long chapter is devoted to correlations and asymptotic properties of the numbers of visits to the different states. The basic tool here is the method of characteristic functions, which leads to the study of the matrix iterates $P(\theta)^n$, $P(\theta)_{ij} = p_{ij} e^{i\theta j}$. In the following chapters this technique is extended to the study of sums of random variables defined on the Markov chain, to more general bivariate chains (X_n, Y_n) . This work of Romanovsky's, which dates back to a paper in 1939, and includes versions of a central limit theorem for all types of processes discussed, seems to have been overlooked by later writers who have analysed such processes in more general cases, though the underlying ideas are similar, and indeed can be traced to Markov's original memoirs. Another novel feature is the long chapter on cyclic chains, which are interpreted broadly to include not only periodic chains, in the usual sense, but also "bicyclic" and "polycyclic" chains, where there are several cycles of states with some group or groups of states in common.

In summary, it is a book of uneven quality, cumbersome and repetitious in many places, superseded in others, overlapping to a large degree with better and more recent books. Nevertheless, it contains ideas which have not been explored in detail elsewhere, and, at least for Western readers, it provides an interesting and hitherto inaccessible link in the Markov chain story. The translation is good, and a small bibliography of more recent work is provided. It is not suitable for a textbook, but as a collector's item is worthy of attention from any serious student of Markov chains.

D. VERE-JONES

¹ Fréchet, M., *Théorie des Événements en Chaîne dans le cas d'un Nombre Fini d'états Possible* (Paris, 1938).

² Feller, W., *Introduction to Probability Theory and its Applications*, 1 (New York, 1949).

³ Chung, K. L., *Markov Processes with Stationary Transition Probabilities* (Berlin, 1960).

Neutron Diffraction

Theory of Thermal Neutron Scattering: The Use of Neutrons for the Investigation of Condensed Matter. By W. Marshall and S. W. Lovesey. Pp. xxiii+599. (Clarendon: Oxford; Oxford University: London, September 1971.) £12.

EXPERIMENTERS in neutron diffraction have from classical times been buoyed up by the rumour that Dr Marshall was writing a book on their subject. Now that the advent season is over, after several false starts, it is not surprising that the product should be looked at with a critical eye and we must be careful not to examine the book from the point of view of what it does not achieve rather than what it does.

The intention of the book is clearly stated in the preface as "to give a comprehensive review of all the theory of thermal neutron scattering that is relevant to the interpretation of experiment" and this it does in a masterly way, but I hope to be forgiven for commenting that, in the main, and there are some exceptions, the interpretation is being carried out by a theorist, whereas I had hoped to find Dr Marshall encouraging the experimenter to operate on a do-it-yourself basis.

Bearing in mind that this is the first book to attempt to bring together in a comprehensive way the intense activity of the past twenty years, interpreting the use of neutron beams for studying solids and liquids, the depth of the treatment is quite remarkable. It would be conveyed scarcely at all by the chapter headings, for these are conventional enough—with the familiar "Nuclear Bragg Scattering" and "Neutron Scattering from Liquids" among them. The overall span of the work can be realized by noting that the topics conjured up by the words nuclear, Bragg, phonons, magnetic, spin-waves, polarization, liquids, gases, critical and defects all receive extensive treatment in rigid, quantitative fashion. A better idea of the depth is given by the fact that the introductory "List of Important Symbols" extends to 258 items and the number of equations judged to be sufficiently important to be identified numerically for subsequent reference is 2,013.

No one who is actively working in the neutron beam field can afford to be without this book or reasonably be ungrateful to the authors for making this immense contribution. The authors make a conscious decision not, in general, to quote the original references to all the ideas and observations which they discuss. This has advantages and disadvantages, and it certainly lessens the value of the author index. A grumble may be justified here, for it seems to be a matter of chance whether an author named in the text is listed.

G. E. BACON

Biological Sciences

Homo

The Roots of Mankind: The Story of Man and his Ancestors. By John Napier. Pp. 240. (George Allen and Unwin: London, October 1971.) Cloth £2.75; paper £1.75.

THE origin and beginning of man remains a subject for myth. Each new piece of evidence finds its place in a variety of world views, old and new, and *Australopithecus* has now joined Gilgamesh and Prometheus as a legendary figure whose past feats as "killer-ape" or social hunter are used to explain our present behaviour by the same magical logic as the feats of any other ancestor figure. John Napier rightly concerns himself almost entirely with the solid facts of anatomy and palaeontology, although the authorities he mentions in his readings on human behaviour are almost entirely confined to modern makers of myths.

The style throughout is determinedly cheerful, and, as a result, highly readable. It is presumably designed for a largely uninformed but genuinely interested undergraduate audience. My main criticism is that it does not distinguish clearly enough between two areas of controversy, both of which are its proper concern. The first is that of the evidence that man is an animal, and an animal extremely like a chimpanzee at that. The second is that of the causes of the evolution of the special features of man.

The arguments for man's animal origins still need to be put to non-biologists and they are given here conveniently and accurately, although I found it confusing that much of the presentation was centred around the difficulties of framing a satisfactory taxonomic definition of *Homo sapiens*. The description of the evolutionary lines of man, apes and old world monkeys is well-balanced, and picks its way expertly amongst the present dissensions over nomenclature and interpretation of the fossil data. Even when it comes to *Homo habilis* and other issues with which the author is professionally concerned, we are told both sides of the argument.

But it is the difference between human evolution and that of other lineages that is now of real interest. It was here that I was dissatisfied. Even in anatomy, where we are, for example, told very clearly what are the special features of the human foot and gait, I felt that it would have been better didactically if their uniqueness and early acquisition in the hominid line had been more stressed.

In areas other than anatomy and

taxonomy, the deficiencies are more important. The origin of human language is clearly one of the two main remaining problems of human evolution. (The other, the evolution of human intellectual abilities, is closely related.) Washoe and the Gardiners have demonstrated that chimpanzees can be taught to use a human symbolic language. Napier cites this fascinating study to show that there is no fundamental difference between man and animals even here. No doubt this needs saying to linguists: it will be interesting to see how well chimpanzees generate a deep structure of grammar in the absence of the genetic basis postulated for it in man by Chomsky.

The study should also be viewed in a quite different way, however. If chimpanzees could talk if they possessed a symbolic language, it makes it doubly significant that they do not. Indeed vocal language is not physically out of the question for non-human primates: I have isolated a full range of vowel sounds from baboon grunts. The special conditions which led to human language are not beyond investigation. The need to plan cooperative hunting was no doubt important. More primitively, vocal mimicry may have been crucial. This ability is widespread amongst birds, where it serves to allow communication with specific individuals (or classes of individual) at a distance.

It would be unfair to finish without stressing that it is only because this is an excellent book, which deals clearly with both fact and speculation within its self-imposed limits, that I have devoted so much space to critical comment. I have no doubt that future editions will allow extension and change.

R. J. ANDREW

Sea Science

Scientists and the Sea, 1650-1900: A Study of Marine Science. By Margaret Deacon. Pp. xvi+445. (Academic Press: London and New York, July 1971.) £5.50; \$16.

OCEANOGRAPHY, as Miss Deacon reminds us, is a descriptive and environmental science. It does not operate within a single paradigm, indeed it relies very much for its development upon advances made in the physical and biological sciences. Partly as a result, oceanography as a science has had something of an identity problem—a problem which only began to resolve itself in the period with which this particular study ends. Miss Deacon's book

therefore deals with marine science in its formative years, and the unity of her study derives, not from the discipline, dedication or research techniques of the scientists themselves, but from the age-old nature of the problems to be solved. Although this book is primarily concerned with the period 1650 to 1900, it wisely begins with an account of scientific thought about the sea in the ancient, mediaeval and renaissance worlds, and herein the problems are posed. What is the cause of the salinity of the oceans? What is the motive force behind the ebb and flow of the tides? What impulse generates the movement of waves? How deep are the oceans and does temperature vary with depth? Where do ocean currents have their origins? These questions perplexed Aristotle, Pliny and Strabo. In the nineteenth century, although the motive force of tides was fairly well understood, these were still the problems marine scientists were grappling with. It is these problems and their solutions which provide the themes of this book.

Interest in marine science, Miss Deacon points out, followed a cyclical pattern: a short burst of activity, and then a long period of stagnation in which much of the accumulated knowledge was lost or distorted. The first major period of activity accompanied the scientific revolution in the seventeenth century and was concentrated in the 1660s. Another came towards the end of the eighteenth century and a third after 1860. In each era a small corps of scientists would digress from their usual field of study to work on marine science. They in turn would interest a wider group of less talented individuals whose principal value was in the collection of statistical data on which reliable research depended. As interest and support for marine science waned, the scientists resignedly returned to their original disciplines, leaving much of their work unfinished.

One of the reasons for this fragmentary development was the obstacles in the way of collecting sufficient and accurate factual material. The development of reliable instruments for recording tidal variations, deep sea temperatures and currents lagged behind the needs of the scientists, and the sea was always a harsh and unreliable working environment. Even apparently quite simple operations such as taking soundings or temperatures were fraught with difficulties; in 1816 it took one hundred of the crew of HMS Eurydice an hour and twenty minutes to haul in 1,435 fathoms of line. The costs of collecting such data placed further barriers in the

way of oceanographic research. No small group of men could themselves amass sufficient information to be able to theorize accurately on the great marine problems of the day. Invariably therefore the scientists fell back on the generosity of the government to provide ships and men. If there was a person in the Admiralty with an interest in marine science—a Pepys or Wharton, for example—the scientists' path was made somewhat easier. Normally governments were unwilling to commit themselves to expenditure which would produce little direct material return.

In the broadest sense the evolution of marine science is shown to parallel the development of science in general. In the second half of the seventeenth century the Aristotelian and Cartesian approach, in which theory outpaced factual knowledge, is eclipsed by the rise of empiricism and the emergence of Newtonian methods. Robert Boyle in particular was concerned that theorizing on subjects such as salinity and tides should follow, not precede, observation and experimentation. In the nineteenth century oceanography gathered strength in line with general interest in other environmental sciences, and indeed profited from advances in those sciences.

Scientists and the Sea deals rather cursorily with developments in marine science in Europe and America, and for this reason it is not a general history of oceanography; rather it puts the history of British marine science in its international context.

Generally the book is lucid and well organized, although the reader emerges at the end of the nineteenth century without a very clear idea of how near to being resolved some of the basic problems of marine science were. It does not always make exciting reading but it is thorough, informative, scholarly and well documented. This is a valuable work on an aspect of the history of science which has been rather neglected hitherto.

D. L. MACKAY

Origin of Blood Cells

Haemopoietic Cells. By D. Metcalf and M. A. S. Moore. Vol. 24. Pp. xiii + 550. (North Holland: Amsterdam and London, 1971.) Hfl. 120; \$35.25.

DURING the past decade available information about the origin and inter-relationships of the cells in the blood, and of factors that regulate their proliferation and differentiation, has greatly increased. This advance has been possible because morphological studies, which had for so long been difficult to interpret, could be supplemented by the use of cell markers (chromosomes or isotopes) and by methods for cloning

cells *in vivo* and *in vitro*. To the latter, in particular, Metcalf and Moore have made notable contributions, and this book is a heroic attempt on their part to review an enormous mass of accumulated information on the origin of haemopoietic cells.

The development of stem cells within the yolk sac blood islands of the embryo seems to be a unique process; it is the only stage when haemopoietic tissue develops from a more undifferentiated precursor population not itself possessing stem cell capacity in the various assay systems used. As yolk-sac haemopoiesis declines, the expanding stem-cell population in foetal liver contributes to the circulating stem-cell pool and leads to colonization of spleen and marrow, while continuing to supply stem cells to primary lymphoid tissue.

The most useful cloning technique has been that of Till and McCulloch, who

in 1961 described the formation in the spleens of irradiated animals, by stem cells occurring in low frequency in bone marrow, of macroscopically visible colonies of differentiating erythroid, granulocytic or megakaryocytic cells. Chromosome markers suggest that the great majority of such colonies are clones, and that since most colonies are of mixed type the stem cells must possess the capacity to differentiate in different directions. It is now clear that the microenvironment of the cell strongly influences the pathway which differentiation will take.

In addition to reviewing detailed evidence in support of these conclusions, Metcalf and Moore discuss a spectrum of factors controlling growth and differentiation, ranging from those that are respectable and well defined, such as erythropoietin, through colony-stimulating factor (or CSF, which un-

Hybrids



Cover picture from the greatly enlarged new edition of *Mammalian Hybrids* by A. P. Gray (Commonwealth Agricultural Bureaux, Slough, 1972; £4.00).

fortunately has the same abbreviation widely used for cerebrospinal fluid) and the thymic humoral factor to chalcones, the effects of which on haemopoiesis are much less well defined. A useful review of genetic defects in haemopoiesis in mice, humans, and what is known of the underlying mechanisms, is also included.

In general, the book is well written and illustrated, and the selection and appraisal of data are fair. Some parts are inevitably weaker than others; for example, the sections on kinetics of proliferation and on humoral factors and cell interactions in immune responses will not meet with the approval of some experts. Much of the information in this book is not yet well known to haematologists and immunologists, and is only beginning to find its way into textbooks. Metcalf and Moore's book is therefore timely, and will become a standard reference to the early period of the renaissance in studies of haemopoiesis.

A. C. ALLISON

Pollen

Pollen: Development and Physiology. Edited by J. Heslop-Harrison. Pp. xi+338. (Butterworth: London, July 1971.) £7.

AFTER the discovery of the basic facts of pollen development and function in the nineteenth century culminating in the discovery of double fertilization, pollen research has been approached from a number of different standpoints with, until recently, only occasional contact between them. This is explained at least in part by the tendency to study pollen as a means to an end. Thus the palynological approach which for many years dominated pollen research looked on the pollen grain as just another spore to be examined in detail and identified. It is true that this approach contributed much to our knowledge of pollen morphology but it contributed even more to plant taxonomy, palaeobotany, the study of hay fever, archaeology and so on. The cytologist, on the other hand, frequently looked at developing pollen mainly as a convenient material for the study of meiosis and mitosis, and there have been many other approaches, often isolated from one another, including the study of the factors affecting fertility such as incompatibility and pollen tube growth in general.

It is usually the development of new techniques which brings about a new impetus and a new integration in a given field of research, and pollen research is no exception. The advent of electron microscopy and a whole complex of new cytochemical and biochemical techniques has brought to-

gether the structural, physiological, biochemical and genetical aspects of pollen research so that in one year we have seen three meetings taking place covering the whole range of pollen developmental studies from microsporogenesis to pollen tube growth.

Pollen: Development and Physiology, edited by J. Heslop-Harrison, is based on these three meetings held in 1969 and is undoubtedly the most important book on pollen to be published in recent years. It contains 25 abstracts from papers and eleven reviews specially revised since the meetings to cover recent advances in most of the main aspects of pollen research and the latter are for me the most interesting and informative part of the book.

From the reviews it can be seen that in the study of pollen development we have a unique opportunity to study within the compass of a few cell generations, and in easily available material, a kaleidoscopic series of changes which can throw light on many problems fundamental to biology. Between pollen mother cell formation and fertilization occur firstly the fundamental reorganization at meiosis which involves not only the change from diploid to haploid but the change from sporophyte to gametophyte. These changes are dealt with admirably in the first two reviews in the section entitled "Nucleus and cytoplasm in microsporogenesis".

The sections which follow cover the multiplicity of changes inherent in the development of the microspore and male gametophyte, changes which embrace not only the pollen itself but also the surrounding sporophyte tissues and the changing relationship between gametophyte and sporophyte, that is, between pollen and tapetum and later between pollen tube and stylar tissue. Three reviews in the second section are devoted entirely to the development, structure and chemistry of the pollen wall and the role of the tapetum, subjects which because of their complexity and the controversy which has surrounded them merit the detailed attention they receive. The remaining three sections are mainly devoted to the later stages of pollen development including the growth of the pollen tube. These six reviews form an important part of the book and deal mainly with the metabolic and fine structural aspects and with the relation between pollen tube and style including incompatibility. This book, with its comprehensive specially extended reviews and bibliographies, is certainly not just another report of proceedings. Nor is it the much needed modern textbook placing the pollen grain together with its female counterpart the embryo sac firmly in its context as the gametophyte of the seed plant. Perhaps it would have been a better

book without the abstracts, many of which are far too brief to be useful to any but those readers who were at the meetings. It will, however, for some time to come serve as a background and a stimulus for future research in this field and because of its general biological interest it should find a place in many personal collections as well as in the libraries.

G. ROY LANE

Under the Water

The Underwater World. By Gwynne Vevers. Pp. 168. (Chatto and Windus: London, November 1971.) £2.

Underwater Science. Edited by J. D. Woods and J. N. Lythgoe. Pp. xiii+330. (Oxford University: London and New York, November 1971.) £4.

THESE two books have somewhat similar titles, but there the resemblance ends.

Vevers's *Underwater World* is a pleasant elementary account of life in waters at all depths with a short introduction to the sea itself, and a final chapter on life in fresh water. In between come sections on plankton, the sea shore, coral reefs, the sea floor, fishes of the open sea, the deep sea, and one on squids, turtles, seals and whales. The whole book is illustrated by black-and-white drawings, many by Lesley Marshall, and others from books by previous authors, but there are one or two misleading mistakes among them, for example, Fig. 27, where the Cypris larva is labelled (b) in the drawing but (c) in the legend. There is much useful information in the book for the non-specialist, but there are some surprising omissions; for example, pages 119 to 122 contain an account of buoyancy problems and how they are met by siphonophores and teleost fish, but there is no mention of the now classic work by Denton, and by him and Gilpin-Brown, on the very different methods used by *Sepia*, *Nautilus* and *Spirula* on the one hand, and by cranchiid and some other squids on the other.

Underwater Science is credited on the dust-jacket and the cover to Woods and Lythgoe, but the contents list reveals that it is in fact a symposium of nine papers by eight authors, the editors contributing (in addition to their admirable editing) a joint introduction on apparatus and methods for the diving scientist and a chapter each on vision by J. N. Lythgoe and on micro-oceanography by J. D. Woods. In this book the term "underwater" means that part of the sea which can be reached by aqualung divers, where sight is the only really important human sense. So after the introduction there are three chapters ("Diver Performance" by A. D. Bad-

deley, "Spatial Perception Underwater" by Helen E. Ross, and "Vision" by J. N. Lythgoe) on the various problems and limitations to be overcome by a diver's eyes. Inevitably these chapters overlap a good deal and require many mutual cross-references, and one cannot help feeling that the balance of the book might be better had they been combined into one joint section. After this there are admirable chapters, full of information, on "Fish Behaviour" (C. C. Hemmings) and "Botany" (E. A. Drew), and then a complete change of subject to "Archaeology" (Joan du Plat Taylor). Miss Taylor deals not only with wrecks from the bronze age, through classical times and up to quite recent ones, and with the exploration of ancient, now submerged, harbour installations, but also with the new techniques in surveying and excavating which have been evolved in the past decade or so. This fascinating chapter is followed by "Geomorphology" by N. C. Fleming, which covers such topics as sea-bed deposits and their movements, and the development and maintenance of underwater caves, clefts and cliffs—information of value to geologists, botanists, zoologists and archaeologists. The book concludes with perhaps the most specialized chapter of all, "Micro-oceanography" by J. D. Woods; this deals mainly with the structure and behaviour of the thermocline off Malta, studied by the release of dyes. This information is not only interesting in itself, but will also be of help to physical oceanographers and planktologists and to divers themselves as underwater animals.

Before concluding, I have two small bones to pick. First, in case after case a graph would have been made more intelligible to the non-specialist (and few if any readers will be specialists in all the many fields covered by this book) if the legends had been longer and more explanatory. Secondly, I take issue with Dr Lythgoe over his summary dismissal (p. 112) of the Secchi disk because its "measurements cannot be interpreted in physical terms". Why on earth should they be? Surely, the insistence that scientific information is of little or no value unless it can be expressed in precise mathematical terms is becoming a form of idolatry. The Secchi disk is simple, cheap and easy to use, and its readings *do* give consistent information of biological significance; for example, whether in the English Channel or on the tropical coast of Queensland a Secchi disk reading will coincide closely with the maximum daylight concentration of zooplankton. This is of real practical importance; if Alain Bombard, on his heroic crossing of the Atlantic in a rubber dinghy, had been able to use a Secchi disk, he could have caught far more fresh food than he did.

Now to sum up: this book is a splendid summary, as of early 1969, of diving science in its early years; no serious diver can afford to be ignorant of it. But all the scientific fields explored in the book are now being ploughed and sown, and they will soon be cropped. In ten or twenty years' time a single volume on "Underwater Science" will almost certainly be impracticable—at least four books will be needed—but in the meantime let us be grateful to editors Lythgoe and Woods for such an admirable introduction.

JOHN S. COLMAN

Development and Genes

Developmental Genetics. By Clement L. Markert and H. Ursprung. Pp. 214. (Prentice-Hall: Englewood Cliffs, New Jersey, November 1971.) \$6.95 cloth; \$3.50 paper.

WE do not know how genes control development. Consequently, the subject is attractive to researchers (and to students), and a relevant paper is published daily and a new review every quarter, or so it seems. It is not a propitious time for textbook writers, and most have been content to deal with only parts of the subject. So Markert and Ursprung's text has been awaited with interest because it promised to survey the whole field and to fill this gap in the literature. Sad to say, it does not come up to expectations. It is out of date (the only 1970 reference is to a collection of earlier papers—and some quoted material is not referenced at all), it is curiously uncertain about its readership and wavers between brief *Scientific American*-type popularization and detailed technicalities, it is poorly written and often lapses into laboratory jargon, and it is sometimes so inadequate as to be misleading. Teachers, subject to the pressure of undergraduate interest, will still have to work hard preparing supplementary reading lists; and graduate students will find the book useful only as a general introduction.

The weakness of the book is that it tries to do too much in too little space. The first three chapters scan the molecular biology of early amphibian and sea urchin development, as shown by RNA and protein synthesis, respectively; and molecular biology is taken up again in chapter 7 which deals with translational and epigenetic control mechanisms (erythroid and lens development). In between, we have a fairly detailed survey of the authors' work on lactic dehydrogenase as an instance of differential gene function; a discussion of the operon concept and the lack of evidence for operator and regulator genes in eukaryotes; a little about posi-

tion-effect variegation, X-inactivation, the effects of hormones (hydrocortisone), and a whole chapter on chromosomal differentiation, including puffing of salivary gland chromosomes and lamp-brush chromosomes. The tissue-specific puffs diagrammed in Fig. 6.3 have not been found in recent work, it may be noted.

The remainder of the text is even more of a "sampler": nucleocytoplasmic interactions (nuclear transplants, cell hybridization, maternal effects—with an error in Fig. 8.11); gene interactions (melanocytes); phenocopies; determination and transdetermination; and regeneration. The penultimate chapter tries to compress the developmental aspects of cancer into half-a-dozen pages; and, following Sonneborn, the last chapter asks: "Omnis embryo e DNA?", but not very seriously.

Genetic manipulation has long been used for the study of metabolism, and this aspect of the subject is rightly stressed. But the crucial questions lie within a narrower area with respect to development, and they have a different character: they concern the coordinated activation and inactivation of batteries of genes, and the mechanisms which regulate this switching in the orderly sequence of normal development. The models we now have for this were proposed after the book was written, but none of them accommodates even all the phenomena which the authors summarize. Since the text does emphasize many of the relevant questions, readers may be encouraged to provide their own answers without the prejudice imposed by implausible models. When we have better theories it will be possible to write a more concise book, and to formulate the problems more exactly.

JAMES H. SANG

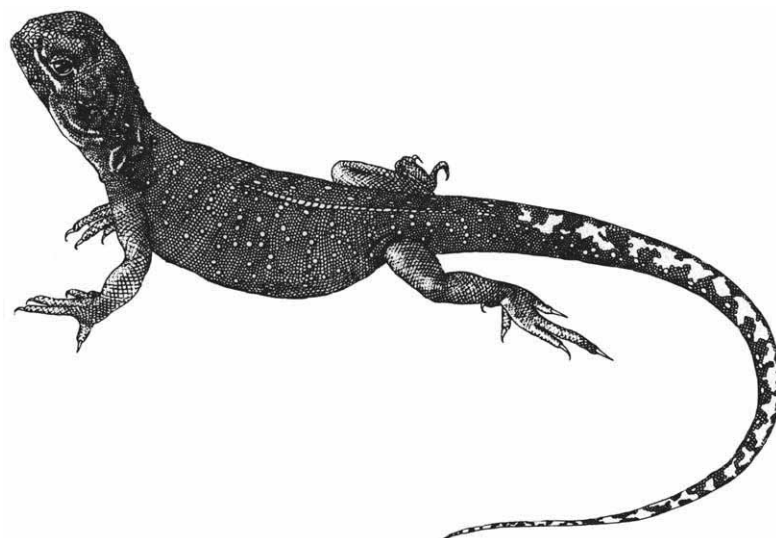
Cell Membranes

Biomembranes. Vol. 1. Edited by Lionel A. Manson. Pp. xiv+293. (Plenum: New York and London, 1971.) \$21.84.

Cell Membranes. Edited by Goetz W. Richter and Dante G. Scarpelli. Pp. ix+180. (Williams and Wilkins: Baltimore, 1971. Distributed in the UK by Churchill Livingstone, Edinburgh.) £7.50.

BOTH these latest additions to the literature on cell membranes had their origins in conferences and, although both contain much of interest to workers in this field, their aims are different. *Biomembranes* is the first volume in a new series to be issued at least once a year, its aim being the "creation of a forum for the lucid exposition of critically

Dragon Lizard



The Dragon Lizards (AGAMIDAE)

A representative of the Agamidae, from *Australian Lizards* by Robert Bustard, which is also illustrated with coloured plates (Collins, Sydney and London, 1972; £3.50).

evaluated opinions and ideas in the area of membrane-associated phenomena". The stimulus for this venture was a conference held at the Wistar Institute and this first issue of *Biomembranes* contains four comprehensive and authoritative reviews which are primarily intended for research workers in these and closely related fields. All four have excellent bibliographies up to 1969 or early 1970 and, if this standard is maintained, *Biomembranes* will be needed on many library shelves. M. R. J. Salton describes current research on bacterial membrane systems, concluding that they are multifunctional structures, although there is evidence that the various functions are distributed and organized in discrete regions. The anatomy and physicochemical properties of bacterial membranes are apparently indistinguishable from those of other cell membranes. The survey of the structure and function of sarcoplasmic reticulum membranes by A. Martonosi concentrates in particular on the biochemistry of Ca^{2+} transport and the role of the ATPase of the sarcoplasmic reticulum. The result is an interesting and closely-argued story. P. M. Kraemer begins his review of the complex carbohydrates of the cell periphery of animal cells with the admission that, whilst one presumes they have a functional role, not a single mammalian cellular complex carbohydrate structure has yet been clearly associated with a physiological cellular function. The problems are revealed in this detailed chapter and among the functions considered are roles in cell division, transport, antigens, receptors, cellular interactions and transformations in malignancy. I felt that both this chapter and the one by Martonosi would have been improved by a concluding section, clarifying the present position for people not intimately associated with the work. L. Warren and M. C. Glick in the final chapter of *Biomembranes* survey the methods available for the isolation of surface membranes from various animal cells, discussing the principles and emphasizing the difficulties, advantages and disadvantages involved in the various techniques. The identification of the sources of membrane vesicles by the use of membrane markers is discussed, but most of this chapter is devoted to techniques relevant for twelve different types of tissue. This is a most useful review for anyone beginning to isolate surface membranes or for workers changing to a new tissue.

Cell Membranes represents the first volume in a series in which the symposia at the annual meeting of the American Association of Pathologists and Bacteriologists are to be published; it consists of nine papers under the general title of biological and pathological aspects of cell membranes. A contro-

versy at the heart of the problems in current studies of cell membranes is the status of the "unit" membrane and the validity of the Danielli-Davson model. Three articles in *Cell Membranes* consider various aspects of this problem and in particular the value of evidence from electron microscopy. F. S. Sjöstrand summarizes the evidence for his model of the molecular structure of mitochondrial membranes, in which the enzyme molecules are arranged in multimolecular aggregates. His thesis is based on the electron microscopic examination of thin sections produced by techniques which reduce the extensive changes of the native conformation of membrane proteins that are caused by conventional fixation and embedding techniques. D. F. Parsons also advocates new approaches to electron microscopy, including electron optical contrast methods and the use of differentially pumped specimen hydration chambers at extra high acceleration voltages. Benedetti and Delbauffe suggest in their review that the configuration of the liver plasma membrane results from the association of at least two main structural components; a triple-layered structure (the backbone) and small globular subunits either attached to the surfaces of the leaflet or spanning it. The chief role of Ca^{2+} is believed to be the maintenance of the triple-layered structure. Other topics covered in *Cell Membranes* include the microsomal drug-hydroxylating enzyme system of liver (Orrenius and Ernster), endocytosis and the fate of endocytized

molecules (Z. A. Cohn), the characterization of the principal glycoprotein of the human erythrocyte membrane (V. T. Marchesi), the infectious cycle of vaccinia (S. Dales) and mitochondrial biogenesis in rat liver cells and its experimental modification by thyroidectomy, riboflavin deficiency and metabolic inhibitors (D. G. Scarpelli *et al.*). A sequence of subcellular alterations following acute lethal injury is proposed by B. F. Trump *et al.*, who emphasize the importance of ion and water movements and of the structure and function of cell membranes; this is a useful survey of the work produced by this group. Some of the contributions to *Cell Membranes* are short and the book reflects the unevenness frequently found in published symposia. Nevertheless, experimental pathologists and research workers in membrane physiology will find items of interest.

C. J. DUNCAN

Nitrogen Fixation

Biological Fixation of Atmospheric Nitrogen. By E. N. Mishustin and V. K. Shil'nikova. Translated by Alan Crozy. Pp. ix+420. (Macmillan: London and Basingstoke, January 1972.) £4.95.

THIS is the English translation of a comprehensive work published in the USSR in 1968, bibliography ending in 1966. It is in some respects out of date, but mostly in biochemical aspects,

which the book does not claim to cover other than superficially and which are therefore relatively unimportant to its aims.

It begins with a survey of the biology, physiology and ecology of the legume symbiosis and a parallel survey of the non-leguminous symbioses. These are excellent and include a lot of relatively inaccessible but useful source material. The discussion of the free-living genus *Azotobacter* is limited, though it includes many useful ecological data, particularly from rather obscure publications. Comprehensive coverage is given to the related bacteria *Beijerinckia* and *Derxia*, however, and a similarly thorough treatment is given to the nitrogen-fixing clostridia.

The chapter on the blue-green algae misses recent work on the function of heterocysts but is very useful concerning the practical value of these organisms. (It includes a curious, dead-pan story about the Goddess of Azolla, responsible for the introduction of *Azolla*, a commensal system of blue-green alga and water fern, in Vietnam. Unhappily the date of the good lady's activities is not given.) The material on "other groups of nitrogen-fixing organisms" is now rather out of date. Nitrogen-fixing strains of *Bacillus*, *Klebsiella*, sulphate-reducing bacteria, coloured sulphur and non-sulphur bacteria are well-authenticated, but few workers wholly accept reports of fixation by yeasts, *Nocardia*, *Pseudomonas*, *Acromobacter* and *Spirillum*, largely because new isolates in these groups prove to be "ghosts" (efficient scavengers of atmospheric fixed nitrogen) and putatively authentic cultures from laboratories or culture collections do not fix. The same is true of the concluding brief survey of the biochemistry of the process, which is so much out of date as to be of little more than historical interest.

Discounting the obsolete portions of the last two chapters, this is the sort of painstaking, comprehensive survey that is rarely produced in the West today. It brings together reports from all sorts of obscure sources as well as from the regular scientific literature. To Western readers it is at times rather uncritical, willing to accept without comment strange claims (for example, that radioactive substances activate nitrogen fixation in *Azotobacter*) and at other times hypercritical (one Sushkina, author of a monograph on *Azotobacter* in 1949, gets severely panned for making an excusable technical mistake in counting azotobacters; an ironical touch is that, since this book was written, another important source of technical error, concerning the use of diluent, has been documented and it is even conceivable that the authors have fallen into this second trap). Specialist students of

Soviet research will be interested to note the authors' view that "azotobacterin", a preparation of azotobacter once heralded in the USSR as a marvellous biological fertilizer, is really of little value and, when it does do some good, it is for reasons unconnected with nitrogen fixation.

The translation is good; a silly mistake has made the running titles of chapters 2 and 3 useless. On its own, this book is perhaps of rather limited value except to specialists but, in conjunction with more recent Western books and reviews, it is essential for libraries where nitrogen fixation is taught or studied in some depth.

JOHN POSTGATE

Coral

Regional Variation in Indian Ocean Coral Reefs. (The Proceedings of a Symposium organized jointly by the Royal Society of London and the Zoological Society of London, May 28 and 29, 1970, No. 28.) Edited by D. R. Stoddart and Sir Maurice Yonge. Pp. xxxv+584. (Academic: New York and London, November 1971.) £9.50; \$28.

DARWIN's famous theory of the origin of coral reefs and coral atolls, already in his mind before and strengthened during the voyage of the *Beagle*, resulted in the spilling of quantities of ink, often acrimoniously, over rival theories of these tropical structures raised by the "greatest of the world's builders". In spite of these rivals it has stood firm and the results of deep drilling on Pacific reefs during the present century have pretty well confirmed the basic validity of Darwin's ideas. The morphology of reefs and the nature and variety of the corals and array of associated animals and plants are now fairly well known, but coral reefs as ecosystems have only in recent years been contemplated as particularly suitable subjects of study because of their virtually closed biosystems.

For many years reefs of the tropical Pacific have received much attention while the prolific reefs of the Indian Ocean and Red Sea have only been lightly surveyed. This imbalance is now changed, as this volume amply demonstrates. It derives from a symposium held in London in May 1970, organized by the Royal Society and the Zoological Society of London under the leadership of D. R. Stoddart and Sir Maurice Yonge, to bring together reports of research on the Indian Ocean reefs during the past few years by some twenty-eight scientists from Great Britain, France, Germany, India, and the USA, who have contributed eighteen papers grouped under the topics Geo-

logy and Morphology of Reefs, Regional Studies of Reefs, Distribution of Corals, and Other Reef Invertebrate Communities, covering the vast area from the Gulf of Aqaba, the Red Sea, the Seychelles, Aldabra, the Maldives, Southern India, Madagascar, the Nicobars, and other sites. Only the extreme eastern Indian ocean receives scant attention.

Space does not permit special consideration of the papers, but one cannot but applaud the first, Sir Maurice Yonge's tribute to the late Thomas F. Goreau, the outstanding coral biologist, whose untimely death occurred only a month before the symposium that he had intended to attend.

The volume itself is very well produced—typographical errors are very rare, the many diagrams, maps, and tables are uniformly clear and well-captioned, although a few of the many half-tones are too muddy to show much; the three indexes have been carefully prepared, and the bibliographies accompanying the papers are unusually complete and up-to-date. This book will be profitably read by the growing coterie of hermatologists, and there is much in it for the biologist, geologist, geographer, and others concerned with the marine sciences. JOHN W. WELLS

Palaeozoogeography

Faunal Provinces in Space and Time. Science editors, F. A. Middlemiss and P. F. Rawson. Technical editor, G. Newall. (Proceedings of Inter-University Geological Congress, held in London, December 17–19, 1969.) Pp. 236. (Seel House: Liverpool, 1971.) £5.50.

AFTER much neglect, the field of palaeozoogeography is now advancing at high speed. It is propelled by the discoveries of global tectonics which demonstrate that continents are in motion themselves, at rates sufficient to alter completely their geographical configurations during Phanerozoic time. This volume attempts to assess the provincial patterns of much of the Phanerozoic, chiefly of the shelf fauna but including pelagic and terrestrial faunas as well, and usually taking the new concepts of continental drift into account.

It begins with a preview chapter which crisply outlines the principal concepts of palaeozoogeography and of global tectonics, and which includes some post-conference literature. Succeeding chapters, each by a separate authority, examine present zoogeography and then the palaeozoogeographic patterns in the fossil record, roughly in stratigraphic order. Although the treatment is inevitably uneven (the late Palaeozoic is

poorly covered) and partly outdated by the rapid march of knowledge the book contains a wealth of synthesized distributional data and conveys a sense of the Phanerozoic patterns of growth and decay of provinciality.

While some chapters are excellent, others are ecologically naive. For example, we are told that salinity is the principal factor underlying marine distributions today, and ecological-evolutionary aspects of population biology, which have proved so fruitful in application to zoogeography, are scarcely mentioned. The concept of a province varies from author to author. Many of the confusions and inconsistencies, however, are rationalized and placed in perspective in the final chapter, which summarizes and evaluates the discussions at the conference, and demonstrates how valuable this volume really is in raising and clarifying many weaknesses of concept and data that remain in the field. The book is handsomely produced with coloured maps and other illustrations.

JAMES W. VALENTINE

Concise Photosynthesis

Biochemistry of Photosynthesis. By R. P. F. Gregory. Pp. xii+202. (Wiley: New York and London. December 1971.) £3.50.

How does one cover the vast field of the biochemistry of photosynthesis? The answer seems to be either to publish symposium and multi-authorship volumes which are usually encyclopaedic and complex or to choose the present course of one author covering the entire field to varying depths. Dr Gregory is to be congratulated on his endeavour to cover the field in 180 pages; this includes introductory sections explaining light phenomena, pigment characteristics, electron transport and redox reactions, and the path of carbon in photosynthesis.

The wealth of information contained in this book is remarkable and an excellent index (one of the best I have come across) makes the data most accessible. Much of the information could have been tabulated for even greater readability and ease of extraction. This is a minor fault; the data are there and are combined with figures, structural formulae and references to give a large amount of information to students and specialists alike.

The first part, comprising a third of the book, provides a very speedy introduction to photosynthesis and would have to be used very selectively; for example, chapter 1 broaches photoautotrophy, carotenoids in photosynthetic bacteria, the Heisenberg uncertainty

principle, and chloroplast structure. The discussion of redox reactions and electron transport is good but the path of carbon is very hastily dealt with and is hardly discussed again in the rest of the book. This section ends with numerical problems and topics for discussion which should be of use to teachers and students.

Part two of the book is primarily about light-induced electron transport with a chapter relating this to the structure of the chloroplast (thylakoid) membranes. This latter very difficult aspect of photosynthesis is well dealt with, even though the field alters continuously. The last chapter deals with the relation of the chloroplast to the rest of the cell. In this part of the book there are some highly complex and erudite discussions which admirably bring in experimental evidence from various sources to support them. However, one does sometimes become confused, for example, when one sees twelve different schemes for non-cyclic and five different schemes for cyclic photophosphorylation! Some drastic pruning and simplification would have helped. Some important topics are dealt with in only a paragraph and there is the surprising omission of the nature and occurrence of ion transport in chloroplasts.

This book should be an asset to all who study or teach photosynthesis—its figures, formulae, accurate data, electron microscope pictures, excellent index, and discussion of chloroplast, mitochondrial and chromatophore reactions from a unifying viewpoint, undoubtedly recommend the book. We should not quibble with what has been omitted, hastily covered, shows bias, or has changed over the last year—if we did no one would write science books, or if they did they would be excessively bland.

D. O. HALL

Testicular Function

The Testis. Volume 3: Influencing Factors. Edited by A. D. Johnson, W. R. Gomes and N. L. Vandemark. Pp. xv+596. (Academic: New York and London, March 1971.) \$36; £16.80.

THIS volume maintains the generally high standard of the first volume, which was reviewed earlier (*Nature*, **231**, 335; 1971).

N. S. Fechner takes a broad view of genetic aspects of testicular development and from an examination of the cytogenetics of sex goes on to review genetics of variations in testis structure and function and of sources of variation in sperm morphology and fertilizing capacity. In his chapter on immunological responses, D. W. Bishop

contributes an interesting and well documented discussion on experimental auto-allergic spermatogenesis. Reference is also made to auto-agglutination and phagocytosis of spermatozoa and the existence of blood group antigens on spermatozoa is discussed.

The effects of the thyroid, adrenal and pineal glands and of gonadal, pituitary and pancreatic hormones, together with some effects of prostaglandins and of antigonadotrophins, are treated by W. R. Gomes, who makes an unexpected digression into recent concepts of hormone action. External influences on testicular function are covered in sections on the effects of pressure and elevation changes by A. J. K. Cockett and A. D. Johnson, and on the effects of temperature by N. L. Vandemark and M. J. Free. As well as discussing the data on the effects of altitude on man and animals, Cockett and Johnson review the experimental evidence on those of atmospheric oxygen, carbon monoxide and neon. Dealing with the effects of space flight, they treat cursorily the data on weightlessness and immobilization and spermatogenesis in some primates and lower species.

Information on the effects of temperature is comprehensively reviewed, with descriptions of the effect of heat on testicular function in animals from fishes to mammals. This chapter includes an examination of effects on lymph flow, gas tensions, carbohydrate, nucleic acid and protein metabolism.

There is a very full discussion of the effects of ischaemia, in which the injury sustained by the ischaemic testis is contrasted with that sustained by the heat-damaged testis. Radiation effects, considered by L. O. Ellis, include those on testis histology and fertility. Factors affecting radiation damage include dosage, route, age, species and physiological and environmental factors. Evidence that irradiation induces a biochemical lesion in the androgen biosynthetic mechanism receives particular attention and there is discussion of possible roles for epinephrine, serotonin, histamine and melatonin in relation to radio protection of the testis.

S. A. Gunn and T. C. Gould give a valuable, comprehensive review of information on the toxic effects of cadmium. In the concluding chapter, W. R. Gomes surveys information about a number of chemical agents affecting testicular function and male fertility, with particular reference to antispermatic agents and those affecting endocrine function.

It is fair to conclude that this book deserves a very high place indeed on any list of books of reference relating to the physiology of mammalian reproduction.

J. L. HANCOCK

Continued from page 274

death rate, that is, the part attributable to the competitor, is plotted, and the starred death rate functions are determined. The curve $d_p^*(N_w)$ behaves strangely at high densities; it appears that *D. willistoni* is buffering the intraspecific effects of *D. pseudoobscura* on itself.

The same strategy is used to obtain the starred recruitment

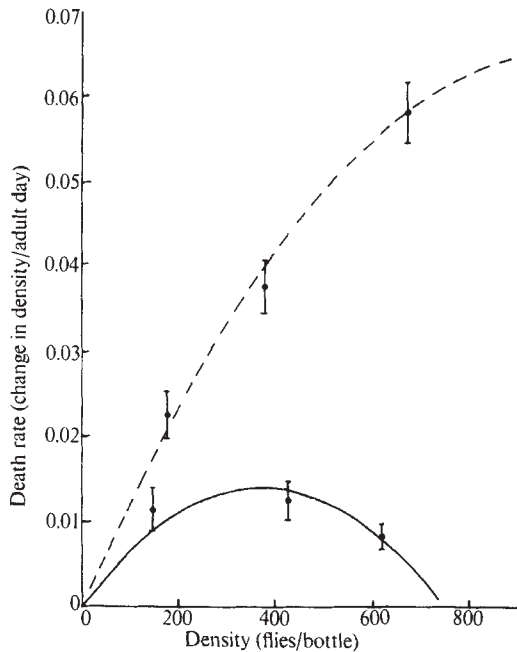


Fig. 4 Estimated functions for the interspecific component of the death rate. ---, Effect of *Drosophila pseudoobscura* on *D. willistoni* ($d_w^*(N_p)$); —, effect of *D. willistoni* on *D. pseudoobscura* ($d_p^*(N_w)$).

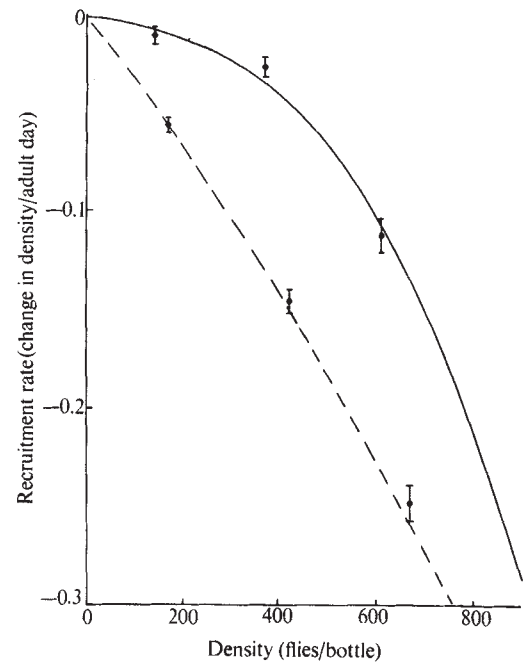


Fig. 5 Estimated functions for the interspecific effect on the recruitment rate. ---, Effect of *Drosophila pseudoobscura* on *D. willistoni* ($b_w^*(N_p)$); —, effect of *D. willistoni* on *D. pseudoobscura* ($b_p^*(N_w)$).

functions (Table 4). The depression of recruitment is plotted against the average competitor population (Fig. 5), and the starred functions are estimated.

System Dynamics

The functions we have obtained permit a complete dynamic description of the *D. pseudoobscura* and *D. willistoni* All

Table 1 Calculation of Intraspecific Death Rate Functions

	<i>D. pseudoobscura</i>				<i>D. willistoni</i>			
Initial density *	200	500	800	1,000	200	500	800	1,000
Survivors after one week *	161 ± 4	397 ± 7	606 ± 16	759 ± 20	154 ± 5	363 ± 14	547 ± 26	650 ± 26
Deaths per week	39 ± 4	103 ± 7	194 ± 16	241 ± 20	46 ± 5	137 ± 14	253 ± 26	350 ± 26
Average survivors through the week	180	446	699	874	176	428	665	813
Deaths per adult per day (d')	0.0305	0.0324	0.0388	0.0386	0.0366	0.0447	0.0528	0.0597
	± 0.0030	± 0.0022	± 0.0027	± 0.0030	± 0.0037	± 0.0046	± 0.0053	± 0.0030
Estimated functions	$d_p'(N_p) = 0.0270 + 0.0000153N_p$				$d_w'(N_w) = 0.0295 + 0.0000364N_w$			

* Ayala's data.

Ayala gives the initial densities and the survivorship. The deaths per week are obtained by subtracting the survivors after one week from the initial density. The weekly death rate is calculated as the week's deaths divided by the initial density, and this is converted to a daily figure by equation (5). The average number of survivors is the average of the integral under the population curve generated by a daily application of the daily death rate. The daily death rate for the population is converted to an individual figure by division by the average number of survivors through the week.

Table 2 Calculation of Intraspecific Recruitment Functions

	<i>D. pseudoobscura</i>				<i>D. willistoni</i>			
Initial density *	200	500	800	1,000	200	500	800	1,000
Weekly production *	306 ± 21	260 ± 22	231 ± 24	172 ± 18	491 ± 25	725 ± 38	728 ± 55	768 ± 28
Recruitment per adult per day (b')	0.243	0.083	0.046	0.028	0.399	0.242	0.157	0.135
	± 0.017	± 0.007	± 0.005	± 0.003	± 0.020	± 0.012	± 0.011	± 0.004
Estimated functions	$b_p'(N_p) = \frac{32.2 \times e^{-0.00094N_p}}{N_p^{0.9083}}$				$b_w'(N_w) = \frac{1.46 \times e^{-0.00125N_w}}{N_w^{0.2085}}$			

* Ayala's data.

The recruitment per day is the result of dividing Ayala's data on production by 7. This yields a per individual figure when divided by the average number of individuals given in Table 1.

system. On a coordinate plane whose axes are the densities of the two populations, N_p and N_w , the curves representing zero growth may be drawn. The curve for N_p is

$$b'_p(N_p) + b'_w(N_w) - d'_p(N_p) - d'_w(N_p) = 0 \quad (7)$$

and similarly for N_w

$$b'_w(N_w) + b'_p(N_p) - d'_w(N_w) - d'_p(N_p) = 0 \quad (8)$$

Since the two lines cross (Fig. 6), there is a point of equilibrium. The frequency of *D. pseudoobscura* at this equilibrium is 31%; this is close to the value 35%, which Ayala observed in his 27 week population experiments. Further, this equilibrium lies below the straight line connecting the two single-species equilibria, K_p and K_w . This agrees with Ayala's observation that the equilibrium is in the zone of strong mutual depression.

In spite of the strong mutual depression, the equilibrium is stable, and coexistence is possible. This can be shown by calculating the dynamic response of the system at points about the equilibrium. But the stability of the equilibrium can also be inferred directly from Fig. 6. As we shall show elsewhere, necessary and sufficient conditions for the stability of a two-species competitive equilibrium are that, at the point of equilibrium, (1) the slope of the zero growth line of the second population (the one plotted along the abscissa) is greater than the slope of the zero growth line of the first population, and (2) both slopes are negative. The lines in Fig. 6 satisfy these criteria. We have also shown (unpublished results) that this is equivalent to the following: a necessary and sufficient condition for the stability of a competitive equilibrium is that

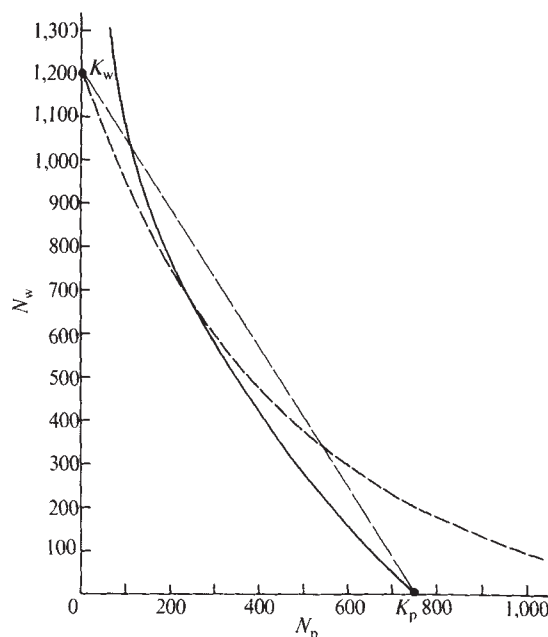


Fig. 6 The determination of the competitive equilibrium. The solid line is the zero growth line for *Drosophila pseudoobscura*, and K_p is its single-species equilibrium. The dashed line is the zero growth line for *D. willistoni* All, and K_w is its single-species equilibrium. The point of equilibrium is at $N_p = 280$ and $N_w = 620$. This occurs in the zone of strong mutual depression. The slope of the *D. willistoni* All zero growth line is the greater at the point of equilibrium.

Table 3 Calculation of Interspecific Death Rate Functions

	<i>D. pseudoobscura</i>			<i>D. willistoni</i>		
Initial density *	200	500	800	200	500	800
Survivors after one week *	152 ± 2	359 ± 8	563 ± 11	101 ± 4	277 ± 8	465 ± 14
Deaths per week	48 ± 2	141 ± 8	237 ± 11	99 ± 4	223 ± 8	335 ± 14
Average survivors through the week	175	426	674	145	377	618
Average competitors through the week	618	377	145	674	426	175
Deaths per adult per day (d)	0.0384	0.0462	0.0490	0.0930	0.0809	0.0746
	± 0.0016	± 0.0023	± 0.0025	± 0.0037	± 0.0032	± 0.0030
Expected intraspecific death rate (d')	0.0297	0.0355	0.0373	0.0348	0.0432	0.0520
Interspecific death rate ($d^* = d - d'$)	0.0084	0.0127	0.0117	0.0582	0.0377	0.0226
	± 0.0016	± 0.0023	± 0.0025	± 0.0037	± 0.0032	± 0.0030
Estimated functions	$d'_p(N_w) = (747N_w - 0.995N_w^2)10^{-7}$			$d'_w(N_p) = (1.3N_p - 0.00065N_p^2)10^{-4}$		

* Ayala's data.

The death rate and average survivorship are calculated as in Table 1. The expected intraspecific death rate is calculated with the starred function estimated from the data of Table 1, and this is subtracted from the deaths per adult per day. This leaves the interspecific death rate. It is used to estimate the interspecific death rate function.

Table 4 Calculation of Interspecific Recruitment Functions

	<i>D. pseudoobscura</i>			<i>D. willistoni</i>		
Initial density *	200	500	800	200	500	800
Average survivors through the week	175	426	674	145	377	618
Average competitors through the week	618	377	145	674	426	175
Weekly production *	168 ± 11	183 ± 14	164 ± 15	186 ± 10	315 ± 19	501 ± 23
Recruitment per adult per day (b)	0.137 ± 0.009	0.061 ± 0.005	0.035 ± 0.003	0.183 ± 0.009	0.119 ± 0.006	0.115 ± 0.004
Expected intraspecific recruitment rate (b')	0.250	0.018	0.046	0.431	0.265	0.176
Interspecific effect on the recruitment rate ($b^* = b - b'$)	-0.113 ± 0.009	-0.027 ± 0.005	-0.011 ± 0.004	-0.248 ± 0.009	-0.146 ± 0.006	-0.056 ± 0.004
Estimated functions	$b'_p(N_w) = -4.66 \times 10^{-5}N_w - 0.333 \times 10^{-9}N_w^3$			$b'_w(N_p) = -3.05 \times 10^{-4}N_p - 1.15 \times 10^{-7}N_p^2$		

* Ayala's data.

Using the numbers for average survivorship from Table 3, the recruitment per adult per day is determined. From this the expected intraspecific recruitment is subtracted, yielding the interspecific effect on the recruitment rate, from which the interspecific recruitment rate function is estimated.

the product of the intraspecific growth regulations be greater than the product of interspecific growth regulations. Consequently, the system seems to be in accord with what both Ayala and Gause would accept as the principle of competitive exclusion.

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Evidence for a New Mechanism of Cell Motility

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Contraction of the stalks of peritrich ciliates depends on a change of state induced by calcium in a rubber-like polymer, without ATP hydrolysis.

THE discovery of actin filaments and myosin-like substances in a wide variety of cells, together with the general requirement of adenosine triphosphate (ATP) for motility, suggests that all motile systems are basically similar. It seems likely that such diverse movements as the beating of flagella, cytoplasmic streaming and the formation of pseudopodia involve a shearing mechanism comparable with that of striated muscle. The possible exceptions to the scheme therefore have a special interest. We have examined one of them, the contraction of peritrich ciliates. In one species, the main contractile organelle or "spasmoneme" is so large that it can be manipulated directly and our first observations suggest that the contraction involves quite different principles.

The spasmoneme, an intracellular organelle, is responsible for the helical coiling or flexure, according to species, of the stalk in vorticellids. It is a rod-shaped proteinaceous body, containing 20–30% dry mass as measured by interference microscopy. In *Vorticella* and *Carchesium*, the chief component visible in the electron microscope is a diffuse filamentous material in which the filaments are of one type approximately 2 nm in diameter^{1,2}. Characteristic tubules, with membranous walls and heterogeneous contents, run longitudinally among the filaments. There is no close structural similarity to muscle and large differences of composition are indicated by the insolubility of the organelle in 4 M KCl, 5 M urea, saturated lithium thiocyanate and sodium thioglycollate. Another clear difference is that the positive birefringence with respect to length of the organelle is substantially reduced during contraction, as observed by Schmidt³ and recently confirmed.

Levine⁴ observed that contraction of glycerinated preparations of *Vorticella* could be induced by divalent ions, and subsequently reversed by the addition of the chelating agent EDTA (ethylenediamine tetraacetic acid). The process

could be repeated many times without adding ATP. Hoffmann-Berling⁵ obtained the greatest effect with calcium and strontium ions. He found that mersalyl, a potent inhibitor of myofibrillar ATPase, was without effect on the contraction induced by calcium, suggesting that ATP was not hydrolysed during contraction. Moreover, the contracted and extended states were maintained indefinitely at the appropriate levels of calcium, although the elastic sheath of the stalk exerts continuous tension on the contracted organelle. These striking differences from muscle have readily been confirmed. It has proved possible to induce preparations of *Vorticella* to perform many cycles of contraction and relaxation by alternate treatment with solutions buffered at 10^{-6} and 10^{-8} g ions l.⁻¹ of calcium, even when mersalyl and metabolic inhibitors were present throughout the process⁶. The response survived storage in a glycerol medium at -20° C for 5 months and was not destroyed by exposure to room temperature for several days and to detergents ('Tween 80' and digitonin). By means of an ethylene glycol bis-N,N-tetraacetate (EGTA) buffer, the effective concentration of calcium for contraction was measured as about 10^{-7} g ions l.⁻¹. Magnesium ions were themselves totally ineffective, even at 2.5×10^{-2} g ions l.⁻¹, and had no effect on the threshold level for calcium.

These experiments posed the question of a source of mechanical energy other than the hydrolysis of ATP. It was suggested that the contraction resulted directly from the binding of calcium, so that the state of the spasmoneme was controlled solely by the availability of calcium ions to the contractile apparatus. In life, the availability could be varied by the cell, for example by actively produced changes in the intracellular level of calcium. The performance of work by glycerinated material, apparently without cost to the preparation, could then arise from the experimental variation in calcium level. The amount of energy which could be converted into mechanical work by such a mechanism depends, *inter alia*, on the amount of calcium that becomes bound to the contractile apparatus and the ratio between the higher and lower concentrations of calcium. It was shown⁶ that enough energy for the mechanical work done by the intact spasmoneme in a single contraction could be provided by the binding of a few micromoles of calcium per ml. of spasmoneme, if the intracellular concentration was varied from 10^{-8} to 10^{-6} g ions l.⁻¹. An

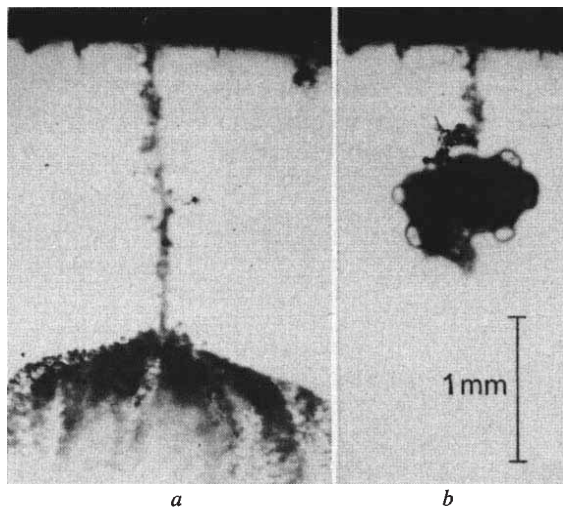


Fig. 1 Living colony of *Zoothamnium geniculatum*, attached to a lily leaf, extended (a) and contracted in response to an electrical stimulus (b). ($\times 18$.)

active variation of this magnitude by a cell is not unreasonable, for the calcium pump of the sarcoplasmic reticulum in striated muscle is capable of working throughout this range of concentration⁷, and membranous structures which might be analogous in function to the reticulum are present in the spasmoneme.

A New Giant Preparation

Quantitative study of the mechanical properties of the spasmoneme of the commoner peritrichs is virtually impossible because the organelle is so small. To investigate this aspect, we have used instead the colonial species *Zoothamnium geniculatum* Ayrton 1903, described by Wesenberg-Lund⁸ and Furssenko⁹, which we have found in Funkedam, a small forest lake near the original Danish locality, and also in slow moving rivers in East Anglia. The members of the colony, or "zooids", are borne on contractile branches. Although the zooids are individually similar in size to *Vorticella*, the common stem, which is shared by several thousand, is much larger. The spasmonemes of the zooids are confluent in the branches, ultimately uniting in the main stem of the colony as a single trunk 1 mm long and 30 μm in diameter. The contractile substance of the colony, physically a single organelle, contracts as a unit. In development, the common stem is laid down at almost its final size by a proportionally large zooid which, before settlement, is a free swimming ciliospore. The ciliospore usually attaches itself to a water plant, often to the underside of a leaf of *Nuphar* or *Nymphaea*, from which the fully grown colony hangs down with the branches extended in the form of an inverted cup (Fig. 1a). In response to vibration the branches close, folding the cup-shaped mass of zooids into a ball. At the same time, the basal half of the stem bends at a definite joint, while the remainder coils into one turn of a helix. The result is that the bunched mass of zooids is whipped upwards against the surface of the lily leaf (Fig. 1b). By high speed filming we have found that these events are completed in only 5 ms. The spasmoneme shortens in this time to approximately 45% of its extended length: the speed of shortening ($110 \text{ lengths s}^{-1}$) is five to ten times higher than in the fastest known striated muscle¹⁰. The subsequent extension takes several seconds. How the movement of the stem is brought about by contraction of the spasmoneme can be understood by considering its structure (Fig. 2). The upper part contains the spasm-

neme, enveloped in cytoplasm densely packed with mitochondria. The path of the spasmoneme is helical in this region, as in *Vorticella*, and the fibrous extracellular material around it is also helically distributed, with the result that the stem bends into a helix when the organelle contracts. The middle region is a hollow fibrous tube, so strengthened that it bends readily at only one point, like a joint in an arthropod limb. The joint is remote from the contractile part of the stalk but it is actuated by an internal tendon which runs to it from the end of the spasmoneme, both joint and tendon being extracellular.

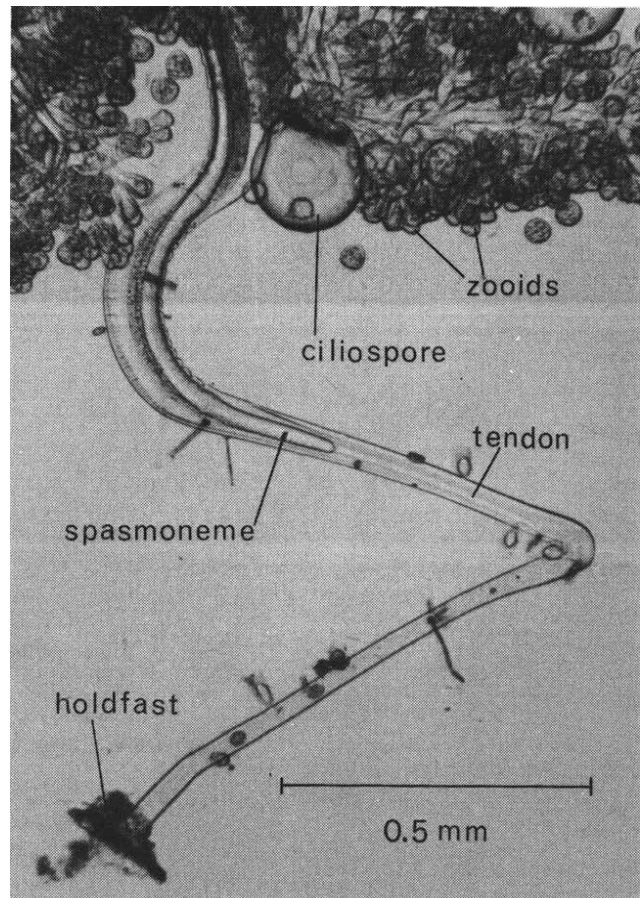


Fig. 2 Stalk region of *Zoothamnium geniculatum*. After treatment with 2% glutaraldehyde the spasmoneme has partially contracted and so induced the stalk to bend at the joint (to the right of the figure). ($\times 80$.)

For our experiments, we have used glycerinated preparations. Mature colonies were detached from *Nymphaea* leaves, rinsed for a few minutes in distilled water and placed in vials containing a glycerol solution at $0-2^\circ \text{C}$. The vials were left at this temperature for 2 h and then stored in a freezer at -20°C for at least a month before use. The glycerol medium contained 50% v/v glycerol in water, with 2 mM Na_2EDTA , 110 mM KCl and 10 mM histidine buffer (pH 7.00) at 20°C . Tris buffer was substituted for histidine without significant effect on the results. Before use, the preparations were allowed to warm to 5°C and rinsed in a similar medium without glycerol. The effects of two standard solutions on the preparations were then investigated. Both contained a CaEGTA buffer with a total buffer concentration of 8 mM, 126 mM KCl and 10 mM histidine buffer (pH 7.00) at 20°C , but they differed in that the proportions of CaEGTA to EGTA were adjusted

to give either 10^{-5} or 10^{-8} g ions l^{-1} of calcium. No change of pH occurred when the solutions were mixed. These solutions will be referred to respectively as the pCa 5 and pCa 8 media ($pCa = -\log_{10} [Ca^{2+}]$).

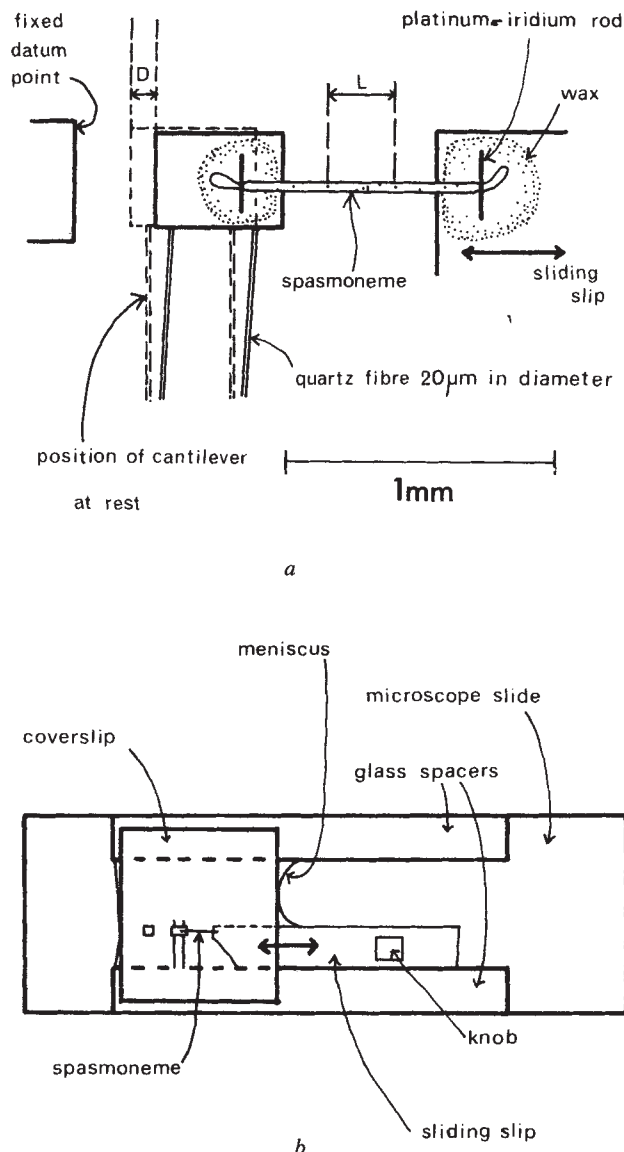


Fig. 3 *a*, Apparatus used to investigate the mechanical properties of the isolated spasmoneme. The length L between graphite markers was measured after active or passive changes in tension, which were followed by observing the deflexion D of the cantilever. *b*, Method of mounting the apparatus for microscopic observation.

At pCa 8 the extracted colonies were fully extended but on transfer to pCa 5 underwent a contraction with flexure of the stalk, as in life, but taking several seconds and without subsequent relaxation. Sometimes the spasmoneme, after contracting, broke free of the tendon and shortened further than in life. The contraction was only partially reversed when the pCa 8 medium was restored, and in this situation the spasmoneme often developed transverse wrinkles, suggesting that extension was occurring in spite of mechanical restrictions imposed by the surrounding material.

To investigate the spasmoneme without such interference, the main stem was cut, and the organelle, with some of the

surrounding cytoplasm, was drawn out of the sheath with fine forceps. The result was a solid rubbery rod, 1 mm long and $30\ \mu m$ in diameter. It proved possible to attach it at one end to a simple microbalance and at the other to a movable support (Fig. 3*a* and *b*). The microbalance consisted of a horizontal glass platform suspended on two quartz fibres $30\ \mu m$ in diameter, the dimensions being chosen so that the sideways deflexion of the platform from its position of rest was almost linearly proportional to the tension in the spasmoneme. The balance was calibrated in absolute units by means of small weights. The movable support, to which the other end of the organelle was attached, was a glass slip, which could be slid on a silicone-greased surface. To ensure linear movement, the slip was held against a guide which was ground flat. Controlled shifts of a few tens of microns could be produced with a finger. At each end the spasmoneme was fastened by trapping it beneath a platinum-iridium rod, which was pressed into a bed of wax (Fig. 3*a*). The apparatus was mounted beneath a coverslip on a microscope slide (Fig. 3*b*), allowing simultaneous observations of length, width,

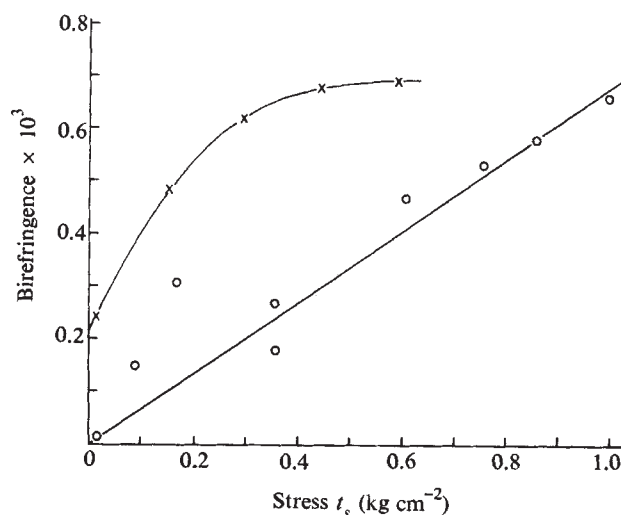


Fig. 4 Dependence of birefringence on stress at pCa 5 ($\circ$) and pCa 8 ($\times$). The stress t_s is expressed in $kg\ cm^{-2}$ of strained cross-sectional area. Figs. 4 and 5 were derived from the same spasmoneme, which had been glycerinated for 1 month.

tension and birefringence, while the bathing solution could be changed and the length varied at will. The length was measured in the central region, using graphite particles placed on the spasmoneme as markers.

Mechanical and Optical Properties

At both high and low calcium levels the spasmoneme showed long-range elasticity. It could be stretched up to almost four times the resting length at pCa 5, with complete elastic recovery. At this calcium concentration the unstrained sample was optically isotropic and became birefringent when extended (see Fig. 4), positive in the direction of stretch. The force-extension curve at pCa 5, shown in Fig. 5, resembles that of a rubber with an elastic modulus G equal to $0.62\ kp\ cm^{-2}$ (or $61 \times 10^3\ N\ m^{-2}$), where G is defined according to the simple kinetic theory for a long-chain rubber, that is $f_s = G v^{1/2} (\lambda - \lambda^{-2})$ in which f_s is the force per unit unstrained, swollen area, $v = 0.15$ is the ratio between the unswollen volume and the swollen volume, and the extension ratio λ is the strained swollen

length divided by the unstrained swollen length¹¹. This indicates that the protein matrix of the spasmoneme behaves as a slightly vulcanized rubber which is ten times softer than resilin¹². In fact, up to $\lambda=2.0$ there is no discrepancy between the measured values and the theoretical curve so that Young's modulus for $\lambda=1.0$ is 0.99 kp cm^{-2} (or $98 \times 10^{-3} \text{ N m}^{-2}$). The birefringence increased linearly with the stress, that is with the force per unit strained area, as in ordinary rubbers, the stress-optical coefficient being $6.5 \times 10^{-4} \text{ cm}^2 \text{ kp}^{-1}$ (or $6.6 \times 10^{-9} \text{ m}^2 \text{ N}^{-1}$) which is four to five times more than in resilin¹². More detailed analyses are obviously necessary but it is clear that the so-called contracted state differs radically from that of an activated muscle.

When kept under zero tension and transferred from $p\text{Ca } 5$ to $p\text{Ca } 8$, the spasmoneme increased in length by approximately 50% and became birefringent, reaching the new equilibrium length within a few minutes after the solution had been changed. As it elongated, the specimen sometimes became bowed and developed a small but measurable negative tension or extending force. At $p\text{Ca } 8$ the preparation followed a new length-tension curve, displaced to the right, as shown in Fig. 5. It was possible to transfer from one curve to the other by changing the solution, so the preparation could be made to follow an anti-clockwise loop on the length-tension diagram and thus to do work. For example the spasmoneme could be taken under zero tension at $p\text{Ca } 8$ and transferred to $p\text{Ca } 5$ at constant length. It would develop a tension of a few mg, which could then be relieved by allowing it to shorten to zero tension and do work. When restored to $p\text{Ca } 8$ and allowed to extend under zero tension, it would reach its initial state.

These properties were shown by preparations glycerinated for up to 3 months. After longer storage, the spasmonemes were less elastic and the response to calcium declined and vanished within a few hours after removal from glycerol. The latter effect took the form of a shift of the length-tension curve at $p\text{Ca } 5$ toward that at $p\text{Ca } 8$, with the curves eventually becoming superimposed.

Interpretation

The observations are the first to be made on the new preparation, and only a tentative theory can be put forward at this stage. Previous work has shown that tension is probably not maintained by a cyclic energy-consuming process: at both high and low calcium levels it can be kept up indefinitely, apparently without an energy supply. Although ATP hydrolysis is probably the ultimate source of energy for contraction *in vivo*, it seems most unlikely that it is necessary for the step immediately leading to contraction or extension in the spasmoneme, since time and inhibitors have so little effect in experiments with glycerinated material, and since the changes can be brought about solely by altering the calcium concentration. At the high calcium level of $p\text{Ca } 5$, the spasmoneme behaves as an isotropic rubber consisting of thermally agitated, kinetically free protein chains which are kept together in a three dimensional network by means of some few, stable crosslinks. The latter is apparent from the relatively low elastic modulus and from the insolubility of the protein. When the concentration is reduced to $p\text{Ca } 8$ the spasmoneme extends and becomes birefringent as if actively stretched but it still remains a soft rubber-like body, although now somewhat anisotropic. It therefore looks as if the change from the contracted to the extended state involves the introduction of additional Ca-sensitive crosslinks in a non-random fashion. This could occur in such a way that the structure is stretched due to internal strains and elastic energy is stored in the network to an extent corresponding to the decrease in configurational entropy of the chains. When calcium ions

again become available the additional bonds are broken and the structure returns to a random network, releasing the stored energy if allowed to shorten. Alternatively, a decrease in configurational entropy during extension might be produced by the mutual repulsion of charges which are left when calcium ions are removed. This could cause the network to expand. These theories are attractive partly because of their simplicity and partly because of the high rate of shortening and mechanical power production of the spasmoneme⁶.

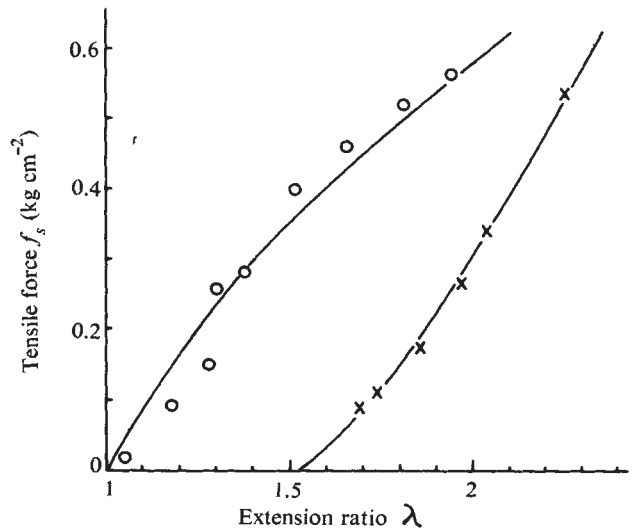


Fig. 5 Force extension curves for a spasmoneme at $p\text{Ca } 5$ (O) and $p\text{Ca } 8$ (X). In both curves, the tensile force f_s is expressed in kg cm^{-2} of the unstrained cross-sectional area at $p\text{Ca } 5$, and the extension ratio is the strained length divided by the unstrained length at $p\text{Ca } 5$. The line through the experimental points for $p\text{Ca } 5$ is a theoretical curve for a long-chain rubber (see text).

It is not known how many essential structural components are present in this motile system. In principle the mechanical function could be achieved by only one, but the slow loss of response to calcium as a consequence of long storage suggests that calcium sensitivity may require a second, diffusible factor. The rubber-like nature of the contracted or extended spasmonemes and the fact that the length-tension curves represent true functions of state clearly differentiate this system from that of muscle and other motile organelles so far described in cells.

We thank Professor Kaj Berg and Mr L. Pastori for help in locating *Zoothamnium geniculatum*. The idea put forward here to explain the behaviour of the spasmoneme resembles that suggested for muscle by the late Dr M. G. M. Pryor¹³.

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LETTERS TO NATURE

PHYSICAL SCIENCES

Teleseismic P Waves from a 10 Ton Explosion

THE Lake Superior experiment¹ showed that the depth at which a shot was fired in water had a very marked effect on the likelihood of detection of that shot at long range. For a particular set of stations the visibility of a 1 ton shot at a depth of about 130 m was about 70% but a variation in depth could cause the detection rate to drop to only 10%. In 1969, an experiment² was carried out in which 135 kg depth charges were fired at depths between 18 and 180 m. The optimum depth was about 91 m, and it was found that the amplitude from a shot fired away from the optimum depth can produce a signal giving a trade amplitude only 40% of the amplitude obtained in the best case. In comparison with the Lake Superior experiment it seems that a relatively small difference in amplitude can have a critical effect on visibility (presumably if the signal level is marginally above noise level).

The optimum depth for an explosion is that at which the first harmonic of the surface reflexion filter (the "reverberation filter" of O'Brien³) coincides with the bubble pulse frequency. As the charge weight is increased the optimum depth increases and the frequency decreases. The WWSS network of stations throughout the world was an obvious target for a speculative test and the peak sensitivity of its short period components lies between 1.4 Hz and 2 Hz. At 1.7 Hz the charge comes out to be 18 tons.

It therefore seemed possible that an explosion which satisfied these conditions might be remarkably effective as a teleseismic source, and an experimental shot was arranged between the Naval Construction Research Establishment (NCRE) at Rosyth, and the Global Seismology Unit of the Institute of Geological Sciences. The biggest charge which NCRE could conveniently handle and suspend from a floating buoy was about 10 tons, and this gave a source peaking at 1.9 Hz. This lies within the required frequency band and is also in a relatively quiet part of the spectrum at many stations.

The explosion was not announced, partly to avoid unnecessary expenditure on an experiment which might not be successful, and partly to provide an idea of the world-wide visibility in normal routine circumstances. The actual charge weight was 10,300 kg of TNT, fired electrically at a depth of 200 m in 210 m of water at 56° 32.5' N, 0° 41' E (North Sea) at 0833 48.25 GMT, on July 20, 1971.

Table 1 shows the magnitudes M_b^* determined from a number of stations less than 10° from the explosion, defined in the equation

$$M_b^* = \log_{10}(V) + 2.3 \log_{10}(R) - 2$$

where V is the ground velocity in $\mu\text{m s}^{-1}$ represented by the maximum P wave amplitude on the record and R is the shot station distance in km (ref. 4). This equation gives a value for M_b determined at short range but it is not yet well tested for this area (the coefficients may need adjustment) and we have pro-

visionally called it M_b^* . The results for fourteen stations give an average value of $M_b^* = 4.2 \pm 0.1$.

Data from nine stations at teleseismic range are shown in Table 2. The magnitudes which are derived for the five stations with amplitude data and $\Delta < 90^\circ$ are more scattered than those of Table 1, but the mean of 4.4 ± 0.4 is not inconsistent with the average value of M_b^* . Evernden⁵ states that a magnitude of 3.5 ± 0.35 should be expected from a 10 ton blast fired in shallow water. The 1969 experiment² indicated that this may be increased by up to 0.4 by firing at the optimum depth so the expected figure based on Evernden's Lake Superior data should be 3.9. This last value is probably subject to an uncertainty of about ± 0.5 unit of magnitude, so that the values obtained in this experiment are higher but not inconsistent with Evernden's data.

If an explosion is to be useful as a teleseismic source it must either be large enough to appear clearly on seismograms because of its sheer size or else, if it is small, it must have some

Table 1 Values of M_b^*

Station		Δ km	M_b^*
EGL	Scotland	226	4.2
EBL	Scotland	248	4.0
EDI	Scotland	250	4.1
EBH	Scotland	262	4.0
EAU	Scotland	269	4.0
EKA	Scotland	276	4.3
ESK	Scotland	279	4.4
EAB	Scotland	314	4.2
BER	Norway	508	4.3
WOL	England	595	4.2
UDD	Sweden	854	4.1
VAL	Ireland	878	4.2
CLL	East Germany	996	4.2
UPP	Sweden	1,060	4.3

Average of 14 values $M_b^* = 4.18 \pm 0.13$.

Table 2 JB Residuals and Some Values of M_b

Station		Δ°	M_b	JB residual (s)
KIC	Ivory Coast	50.2	—	-3.4
YKA	Canada	51.1	—	-2.4
GLM	Alaska	56.4	4.4	-3.5
LAO	Montana	60.5	3.8	-2.6*
BMO	Oregon	66.4	4.2	-3.9*
UBO	Utah	67.1	4.8	-2.7*
JAS	California	73.5	—	-1.6
BRL	Brazil	82.7	4.7	-2.1
BRS	Australia	144.7	—	-0.5 (PKP)

* From NOAA.

Average of five values $M_b = 4.38 \pm 0.40$.

characteristic or signature that will make it distinguishable. The 10 ton blast comes in the second category. Because it has a predominant period of 1.9 Hz and a magnitude between 4 and 4.5 it stands a marginal chance of being widely observed on WWSSN stations. Replies to a questionnaire sent out in August suggest that very few stations noticed it, so a final evaluation of how widely visible it was will have to await a study of original records as they become available, and of a set of WWSSN microfilms which have been ordered. However, it was well observed at Brasilia ($\Delta = 82.7^\circ$) and as a small signal at the PKP caustic (BRS, $\Delta = 144.7^\circ$).

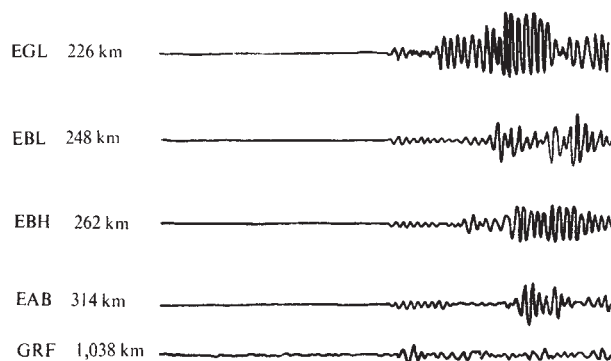


Fig. 1 Some seismograms from stations less than 10° from the explosion.

Preliminary record sections have been made by aligning 30 s extracts from original seismograms of individual short-period vertical instruments, centred on the clear arrivals at short range, and on the arrival times expected from the JB tables at long ranges. For $\Delta < 10^\circ$ (Fig. 1) the signal is fairly clear and at the closer stations, notably EGL, waves with the period of the third harmonic of the reverberation filter can be seen ($T = 0.18$ s). At GRF the signal is beginning to appear more like that to be seen in the long-range group.

Fig. 2 shows signals at distances in excess of 50° . At KIC the signal appears as a small oscillation on a background of much longer period microseisms. At YKA, at almost the same distance, a much clearer record has been obtained by filtering

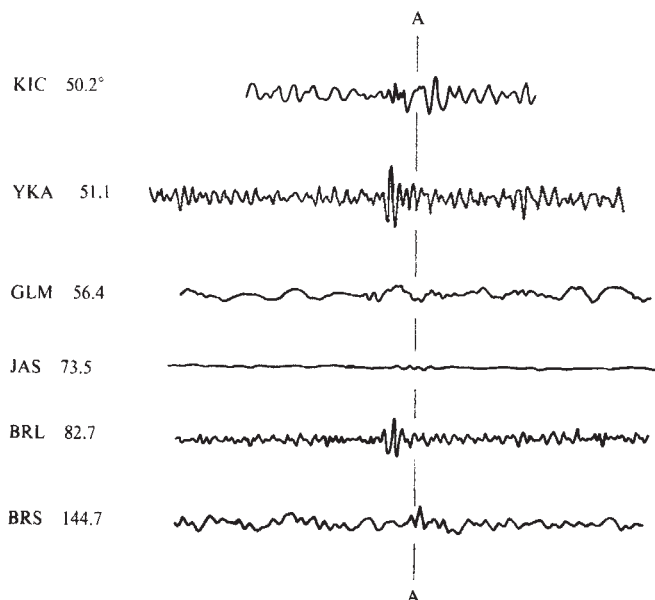


Fig. 2 Seismograms from stations at distances greater than 50° . The line AA marks the expected time of arrival according to the Jeffreys-Bullen tables. Each record section is 30 s long.

from magnetic tape. At GLM the signal is also superimposed on microseisms. At JAS (response not known) the signal is clearly present but small on a low background. BRL (Brasilia Array) is at a greater range than any of the previous four, and again shows the effectiveness of filtering out the longer period microseisms. Finally, the characteristic short burst of two or three waves is seen on the record from Brisbane (BRS). In all cases the dominant period is close to the predicted value of 1.9 Hz. Fig. 3 shows the result of phased summation of the elements of the YKA array.

The detonation of this comparatively small charge has confirmed that it does constitute a sufficiently powerful seismic source to obtain world-wide observations in optimum conditions, and raises the possibility of a substantial number of such explosions, fired within a few weeks of each other by the navies or oceanographic institutes of different countries.

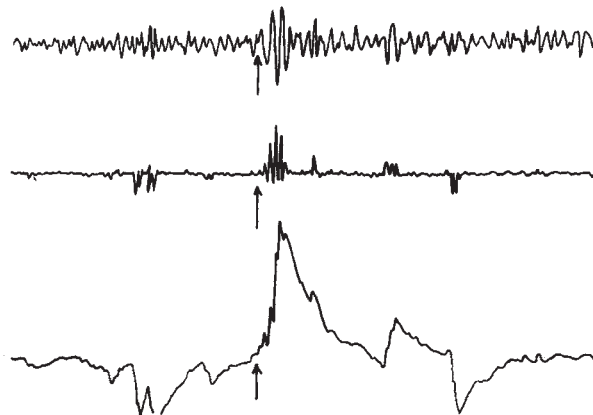


Fig. 3 Array processor output from YKA. The upper trace shows outputs for one arm (ten seismometers, 2.5 km apart) summed for a velocity of 14.7 km/s. The second trace shows the product of the two perpendicular arms. The bottom trace shows the product summed over a moving 1-s window. (Output by courtesy of UK Atomic Energy Authority.)

The International Union of Geodesy and Geophysics established a Working Group at its recent General Assembly to look into this possibility, but the margin of success of the preliminary experiment was hardly sufficient to enable us to recommend a full scale follow-up without further investigation. The options are to test the reliability of observing 10 ton shots with advanced tape-recording systems, and to test the ability of conventional stations to record somewhat larger charges (say, 50 tons). If, as has been suggested⁶, there are low-Q regions at depths of 500 km or more under certain areas, then a signal with a period as high as 1.9 Hz may be lost through attenuation. If so, there would be a compensating advantage, arising from the value of the experiment in mapping such low-Q regions.

We thank NCRE personnel, in particular the explosives officer Mr C. C. Moore and the captain of the Barfoot, Mr Owens. We also thank Mr P. D. Marshall of UKAEA for discussions and for drawing our attention to ref. 4.

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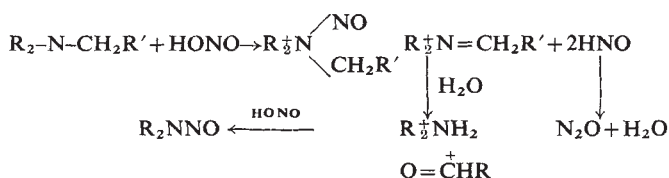
BIOLOGICAL SCIENCES

Formation of N-Nitrosodimethylamine from Naturally Occurring Quaternary Ammonium Compounds and Tertiary Amines

THE potential for the formation of N-nitrosamines in the human environment, either during the processing of foods or *in vivo* from nitrate and/or nitrite, and precursor amines has received considerable attention in recent years. Traditionally only secondary amines were thought to undergo N-nitrosation reactions. While secondary amines *per se* are not common in biological systems, tertiary amines and quaternary ammonium compounds do occur in plant and animal tissue. The possible formation of N-nitrosamines from these compounds must be considered because recent reports have appeared on the nitrosation of trimethylamine^{1,2} and trimethylamine oxide¹ in connexion with the possible formation of N-nitrosamines in fish products cured with nitrate. The nitrosative cleavage of tertiary amines is not new and has been described before^{3,4}.

We are reporting the formation of dimethylnitrosamine (DMNA) from several quaternary ammonium compounds and some of their related tertiary amines in conditions simulating those found in comminuted meat products. The N-containing compounds (4.56 mmol) were reacted with NaNO₂ (22.8 mmol) in pH 5.6 buffer for 4 h at 78° C, then extracted with CH₂Cl₂; the extracts were dried with anhydrous Na₂SO₄ and concentrated in a Kuderna-Danish concentrator. DMNA was determined by a Varian aerograph model 1520 gas-liquid chromatograph using FID. The GLC conditions are as follows: column—2 feet × 1/8 inch absorbance 100–120 mesh 'Chromosorb 101'; flow rates (cm³ min⁻¹)—He 102, air 429, H₂ 57; temperatures (°C)—detector 218; injector port 185°, column isothermal 150°. A gas-liquid chromatography-mass spectroscopy (GLC-MS) system with a DuPont model 492 mass spectrometer was used for all confirmation of identification. The results are shown in Table 1. DMNA is produced from tetramethylammonium chloride at almost the same level as from trimethylamine. The quaternary ammonium compound is demethylated, as shown by the accumulation of

trimethylamine in the absence of nitrite. Smith⁴ has proposed a pathway for the N-nitrosation of tertiary amines that appears to be applicable in this case:



The naturally occurring quaternary ammonium compounds, neurine, carnitine, betaine, choline and acetylcholine, also yield DMNA when reacted with NaNO₂ although at a considerably lower level than the tetramethylammonium compound. The results reflect the ability of the quaternary compound to dealkylate to trimethylamine or a mixed amine containing the Me₂N- group and another substituent. N,N-Dimethylglycine and its methyl ester are tertiary amines derived from the rearrangement of betaine in acidic conditions; 2-dimethylaminoethanol and its acetate derivative are similarly related to choline and acetylcholine respectively. The DMNA yield from the mixed alkyl tertiary amines is two to five times that obtained from trimethylamine and 4 × 10³ to 2 × 10⁴ times the DMNA formed from the parent quaternary ammonium compounds. In fact, 2-dimethylaminoethyl acetate forms DMNA almost one-half that formed by dimethylamine. It is interesting to note that larger quantities of DMNA are produced from the esters of the quaternary and tertiary compounds than from the parent compounds.

It is significant that DMNA can be formed from quaternary ammonium compounds, albeit at low levels, in the conditions described. We can therefore conclude that although quaternary ammonium compounds may represent a potential source of nitrosamine, the tertiary amines and free secondary amines present in food products are of greater concern.

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N-Dimethylnitrosamine in Tobacco Smoke Condensate

THE possible presence of N-nitrosamines in cigarette smoke and/or condensate has been of interest for some years. Druckrey and Preussmann¹ postulated that N-nitrosamines could be produced in burning tobacco, but the difficulty of measuring these compounds prevented the testing of this postulate. We have developed² a gas chromatographic method for the selective detection of N-nitrosamines, and now report its application to the analysis of cigarette smoke condensate. We have identified N-dimethylnitrosamine (DMN) in cigarette smoke condensate, and shown that certain tobaccos grown in different conditions vary widely in their content of DMN.

Table 1 Formation of N-Nitrosodimethylamine from Naturally Occurring Quaternary Ammonium Compounds and Tertiary Amines

Structure	Name	mg DMNA mol amine*
(CH ₃) ₂ NH.HCl	Dimethylamine . HCl	7,108
(CH ₃) ₃ N.HCl	Trimethylamine . HCl	643
(CH ₃) ₄ NCl ⁺	Tetramethylammonium chloride	433
(CH ₃) ₃ N ⁺ -CH=CH ₂ Cl	Neurine chloride	133
(CH ₃) ₃ N ⁺ -CH ₂ CH ₂ OAcCl ⁻	Acetylcholine chloride	0.897
(CH ₃) ₃ N ⁺ -CH ₂ CH ₂ OHCl ⁻	Choline chloride	0.160
(CH ₃) ₃ N ⁺ -CH ₂ CO ₂ ⁻	Betaine	0.159
(CH ₃) ₃ N ⁺ -CH ₂ CH(OH)CH ₂ CO ₂ HCl ⁻	Carnitine chloride	0.044
(CH ₃) ₂ N-CH ₂ CH ₂ OAc	2-Dimethylaminoethyl acetate	3,524
(CH ₃) ₂ N-CH ₂ CH ₂ OH	2-Dimethylamino- ethanol	1,179
(CH ₃) ₂ N-CH ₂ CO ₂ Me	Methyl ester of N,N- dimethylglycine	3,004
(CH ₃) ₂ N-CH ₂ CO ₂ H	N,N-Dimethylglycine	1,405

* Quantification based on the use of cyclohexanone as an internal standard.

The principal interferences in smoke condensate are amines which can easily be converted to acid chlorides and separated from the N-nitrosamines by classical methods. It is then possible to obtain the volatile N-nitrosamines, free of other amines, in dichloromethane (DCM). After evaporation of excess DCM, aliquots of the residue can be injected into a gas chromatograph with the selective detection system. The details of the method for sample preparation will be published later.

The chromatograms of smoke condensate thus obtained show two peaks with retention times identical to N-dimethylnitrosamine and N-methylethylnitrosamine on two different gas chromatographic column packings: 10% 'Carbowax 1540' on 'Gas-Chrom Q' and 'Porapak Q'. The compound responsible for the larger peak has been isolated and identified by high resolution mass spectroscopy as N-dimethylnitrosamine, confirming the presence of this compound in cigarette smoke condensate.

Nitrosamine analyses have been made on the condensates from a popular brand of non-filter cigarettes and on experimental cigarettes. The latter were provided by the US Department of Agriculture, and made from individual tobaccos rather than blended tobaccos. Each tobacco had been grown in both a "low"-nitrogen soil and a "high"-nitrogen soil.

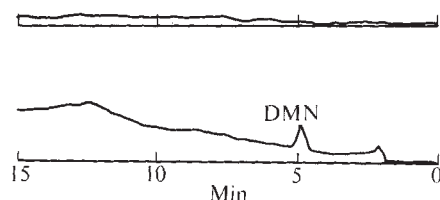


Fig. 1 Chromatograms of Robinson-low nitrogen (upper) and Robinson-low nitrogen (lower) 10 ng/cigarette spike, condensates.

In each case, twenty cigarettes were smoked, the condensate collected and the DCM concentrate obtained. A 40 μ l. aliquot of the concentrate was injected into the chromatograph so that all samples were directly comparable. The DMN contents of the condensates collected varied from 0 to 140 ng/cigarette and were as in Table 1.

Table 1 DMN in Concentrates

Tobacco	Nitrogen in soil	DMN (μ g/cigarette)
Robinson	Low	0
Catterton	Low	5
Burley	Low	3
Robinson	High	27
Catterton	High	60
Burley	High	140
Popular brand	Non-filter	8

These data were obtained by spiking samples of the Robinson-low nitrogen condensate with 100 ng/cigarette and 10 ng/cigarette of N-dimethylnitrosamine and comparing the average response of the spiked samples with the response obtained with the various other condensate samples. Fig. 1 shows the chromatograms of the Robinson-low nitrogen unspiked and the Robinson-low nitrogen spiked at 10 ng/cigarette. Fig. 2 shows that the Burley-high nitrogen condensate had the highest amount of N-dimethylnitrosamine.

Although only a limited amount of work has been done on the N-nitrosamines in cigarette condensate as a function of tobacco variety and soil growing conditions, it seems that these factors markedly affect the N-nitrosamine production. This

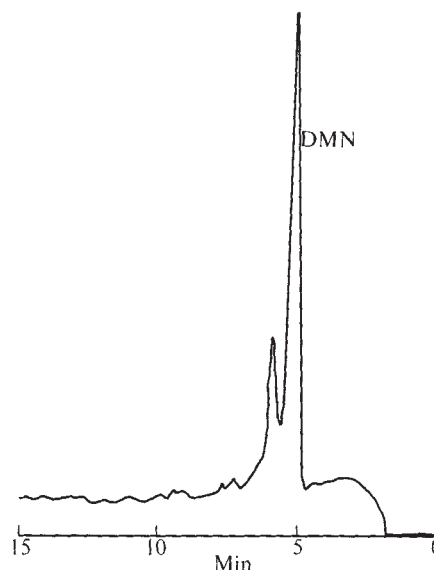


Fig. 2 Chromatogram of Burley-high nitrogen condensate.

type of information could lead to cigarettes with a minimum of N-nitrosamines.

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Cigarette Smoking and Plasma Levels of Nicotine

THE changes in blood levels and tissue distribution of nicotine and its metabolites have been studied in experimental animals after administration of tobacco smoke or smoking doses of nicotine¹⁻³, but there is no comparable information for man. Such data are important in ascribing a role to nicotine in smoking. Recently, methods have been described for measurement of nicotine levels in human venous blood^{4,5}, but they are complicated and have not been used to follow changes in blood levels of nicotine in any detail. We have improved our method¹ by using the newly developed alkali flame ionization detector⁶ (AFID) with the gas-liquid chromatograph (GLC) so that it is sensitive to 1 ng/ml. of nicotine in a 2.5 ml. sample and have used it for assay of plasma nicotine.

Samples of about 10 ml. of blood were taken from the antecubital vein and 2.5 ml. of plasma was mixed with 3 ml. of an aqueous solution of 15 ng/ml. of modaline as internal standard and 1 ml. of 5 M sodium hydroxide. The mixture was extracted three times with diethyl ether, and the combined ether extracts (11 ml.) were placed in a 15 ml. glass stoppered tube, the end of which was formed into a capillary of about

75 μ l. capacity. The volume was carefully reduced to about 20 μ l., initially on a 42° C water bath and finally at room temperature. An ice bath was used several times during the evaporation to chill the tube so that the walls were washed down with condensed ether. An aliquot of 2.0 μ l. was injected into the port of a Varian model 1400 GLC fitted with a Varian AFID which is selectively sensitive to organic compounds containing phosphorous or nitrogen atoms, and is 10,000 times less sensitive to hydrocarbons than the standard FID⁷. With this detector, background noise due to column bleed and tailing of the solvent peak was reduced and the electrometer of the GLC was operated at its maximum sensitivity (1×10^{-12} A/mV). The column was 5 feet $\times$ 1/8 inch stainless steel packed with 80/100 mesh Varaport 30 coated with 6.5% 'Carbowax' 20 M and 13% potassium hydroxide. Gas flow rates were: nitrogen, 30 ml./min; hydrogen, 27.7 ml./min; air, 233 ml./min. The column oven was held at 146° C and the injector and detector ovens at 250° C. In these conditions, the minimum detectable quantity of nicotine was about 0.2 ng and the retention times were 8.3 min for nicotine and 12 min for the internal standard. The ether gave rise to several peaks, but these did not interfere with the assay.

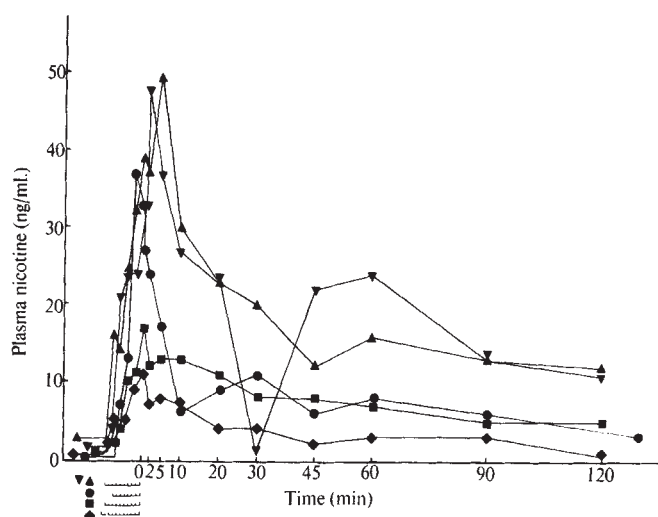


Fig. 1 Effect of a single cigarette on plasma nicotine concentration in four subjects: P. H. (two experiments), (a) ∇ and (b) $\blacktriangle$; M. R., $\bullet$; J. G., $\blacksquare$; B. E., $\blacklozenge$. The number of puffs taken by each is shown beneath the abscissa, the curves being arranged as coincident on the last puff which is assigned to zero time on the abscissa. Blood samples were taken before smoking, 30 s after every second puff, and at 0.5, 2, 5, 10, 20, 30, 45, 60, 90 and 120 min after the last puff. The pre-smoking nicotine levels are the means of assays on two samples taken 5 min apart.

Extracts of plasma from ten male and four female non-smokers contained a peak with the same retention time as nicotine, but this peak was small, being 6–17% that of the internal standard. Calibration curves were prepared for each experiment with smokers by adding nicotine to non-smokers' plasma in concentrations of 5 to 36 ng/ml. The plot of the resulting peak height ratio (nicotine/modaline) against the concentration of added nicotine was linear, but did not pass through the origin.

Blood samples were taken from six male habitual smokers, all of whom inhaled, after they had refrained from smoking for at least 8 h previously (overnight). The subject then smoked his preferred brand of cigarette *ad libitum* for 6.5 h. A second blood sample was taken 30 min after the last cigarette. All the subjects had considerably higher plasma levels of nicotine after the period of smoking (Table 1), and the rise was related to the amount of nicotine presented per unit body weight, as calculated from the numbers of cigarettes smoked and the standard nicotine yield ($r = 0.88$, $P \approx 0.02$).

Blood samples were taken from subjects who had refrained from smoking for at least 8 h. The subjects then smoked their preferred brands of cigarettes to a butt length of 23 mm or filter length plus 8 mm, whichever was greater. They were instructed to take puffs and inhale in their usual manner, but the rate was controlled to one puff/min. Blood samples were taken during smoking and at intervals after the last puff for up to 2 h.

Plasma nicotine levels increased rapidly during the smoking of the cigarette and thereafter gradually fell (Fig. 1). However, there were considerable differences between the subjects which may be attributed to differences in the amounts of nicotine in the smoke, the extents and rates of absorption and the mechanisms by which nicotine is lost from plasma

Table 1 Plasma Nicotine Levels before and 30 min after 6.5 h of *ad libitum* Smoking

Subject (body weight, kg)	No. of cigarettes smoked	Brand of cigarette	Nicotine yield* (mg)	Plasma nicotine (ng/ml.)	
				Before smoking	After smoking
P. H. (60)	11	A (P)	1.3	8	44
M. R. (89)	14	A (P)	1.3	3	31
J. G. (66)	5	B (F)	1.5	2	16
B. E. (71)	12	C (F)	0.5	1	12
J. L. (84)	10	C (F)	0.5	2	21

* Mean values determined on a standard smoking machine; 35 ml. puffs of 2 s duration/min to a butt length of 30 mm. Data supplied by British Tobacco Co. (Australia) Ltd.
P, Plain; F, filter tips.

Table 2 Amounts of Nicotine Abstracted from Smoke

	Subject	Cigarette	Nicotine in smoke (mg)		Nicotine abstracted from smoke (mg)
			Inhaled smoke*	Exhaled smoke	
One cigarette	P. H. (a)	A	3.15 ± 0.23	0.03	3.12
	(b)	A	3.17 ± 0.04	0.09	3.08
	M. R.	A	2.75 ± 0.08	0.68	2.07
	J. G.	B	1.57 ± 0.09	0.00	1.57
Five cigarettes	B. E.	C	0.86 ± 0.07	0.00	0.86
	R. M. (77 kg)	B	$5 \times (1.2 \pm 0.10)$	0.08	5.92
	M. R.	A	$5 \times (2.43 \pm 0.32)$	0.29	11.86

* Mean $\pm$ s.e. from three determinations.

The amount of nicotine abstracted from the smoke by each subject was calculated from the difference between the nicotine contents of exhaled and inhaled smoke (Table 2). During the experiment, each puff was exhaled through a Cambridge filter which retained 99.9% of the nicotine in smoke⁸. Subsequently, each subject smoked three cigarettes from the same packet through Cambridge filters in the same way as he smoked the experimental cigarette. The filter pads were assayed for nicotine¹. The amount of nicotine abstracted from a cigarette by a smoker may be considerably different from the nicotine yield of the same brand of cigarette as measured with a standard smoking machine (Tables 1 and 2). In the experiments with smokers, the puff size, "puff profile" and butt length differed from those with the smoking machine. The maximal level to which the plasma nicotine rose was related to the amount of nicotine abstracted ($r = 0.88$, $P \approx 0.02$). The amounts of exhaled nicotine varied between subjects and this may reflect differences in the pattern of admixture of smoke with air in inhalation and hence the level of the respiratory tree to which the smoke was carried.

The rate of decline of the plasma levels of nicotine varied between smokers, however; the plasma half-life was less than 30 min in all four subjects. It seems that the decay curve is composed of two components; an initial rapid phase which is probably due to uptake of nicotine from the blood into various tissues⁹, and a slower phase which may represent metabolism and excretion of nicotine.

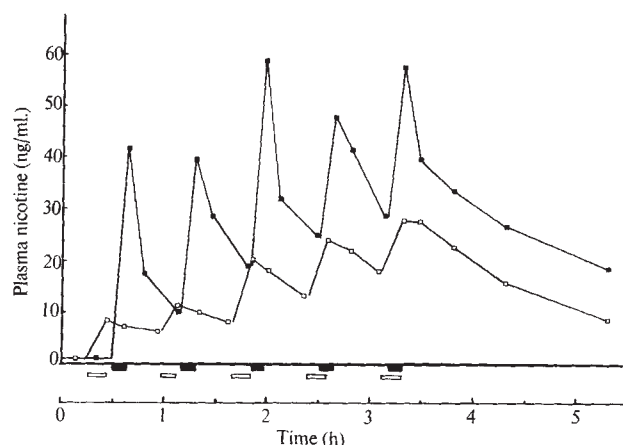


Fig. 2 Effects of five consecutive cigarettes on plasma nicotine concentration in two subjects; R. M., $\square$; M. R., $\blacksquare$. Thirty min elapsed between the end of one cigarette and the start of the next. The periods of smoking each cigarette are shown as $\square$ for R. M., and $\blacksquare$ for M. R. The curves are brought to coincidence at the end of smoking of the fifth cigarette. Blood samples were taken before smoking, at 0.5, 10 and 30 min after the last puff of each cigarette and also at 60 and 120 min after the fifth cigarette.

The observed changes in plasma nicotine levels after smoking a single cigarette suggest that the plasma levels after *ad libitum* smoking (Table 1) reflected the accumulation of nicotine from earlier cigarettes as well as the immediate influence of the final one. The effect of smoking five cigarettes with a 30 min period between finishing one and commencing the next was therefore studied in two subjects (Fig. 2). Although there were considerable differences between them in terms of the nicotine abstracted (Table 2) and the maximal concentration of the plasma nicotine produced by each cigarette, both show a similar trend in the cumulation of nicotine. In one subject (M. R.) the plasma nicotine concentration approached a plateau level and 30 min after the fifth cigarette it was 33 ng/ml. which is close to the concentration (31 ng/ml.) measured 30 min after the fourteenth cigarette of a series smoked at the same average frequency (Table 1).

In spite of the cumulation of plasma nicotine during a day of smoking, it seems that the rate of elimination is high enough to prevent any appreciable cumulation from one day to the next. In six of the twelve instances on which plasma of habitual smokers was examined before smoking had commenced for the day, and at least 8 h had elapsed since the last cigarette of the previous day, the ratios for the peak with the same retention time as nicotine compared with that of the internal standard fell within the range found for non-smokers (6–17%). On five of the other six occasions the peak height ratios were within the range of 18–22%. In only one case was it well outside the threshold range, with a value of 37%; and this subject had smoked particularly heavily at a social occasion on the previous evening.

The method described is adequate, in terms of sensitivity, specificity, rapidity and amount of blood required per sample, for carrying out detailed studies of plasma nicotine levels in cigarette smokers. It remains to establish how the patterns presented here relate to those occurring in a wider population of cigarette smokers, including smokers who do not inhale, and in users of other forms of tobacco.

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Changes in Human Serum Dopamine- β -Hydroxylase Activity with Age

DOPAMINE- β -HYDROXYLASE (D β H), the enzyme which catalyses the last step in norepinephrine biosynthesis, is localized in the norepinephrine containing granules of adrenal medulla¹ and in storage particles of the sympathetic innervated tissues². It is also present in the blood vessel walls and accumulates adjacent to the walls^{3,4}. Stimulation of the nerves of isolated spleen releases D β H into the perfusate^{5,6}. Evidence has been presented that D β H is released from adrenal medulla by exocytosis, a process which expels from the cell the entire content of the catecholamine storage vesicles⁷. But adrenalectomy does not lower the serum D β H levels in rats (M. G. and L. S. F., unpublished observation) so it seems likely that most of the circulating D β H derives from extramedullary tissues.

The delicate development of a sensitive assay system has permitted detection of D β H in serum^{8–11}. This study was carried out to determine serum D β H levels in a group of normal subjects of different ages, to determine whether serum D β H activity varies with age and whether these variations must be taken into account when serum D β H activity assay is used for clinical purposes.

Sera were diluted 50–100-fold and the D β H activity determined by a procedure which has been described^{8,9}. One unit is defined as the enzyme activity that forms 1 nmol of product.

The blood level is clearly influenced by age (Fig. 1). There is a wide range of activities in all groups but there is clearly a progression with age from a preponderance of sera with low activities to sera with high activities. This is reflected in the means which reveal an increase with each successive age group.

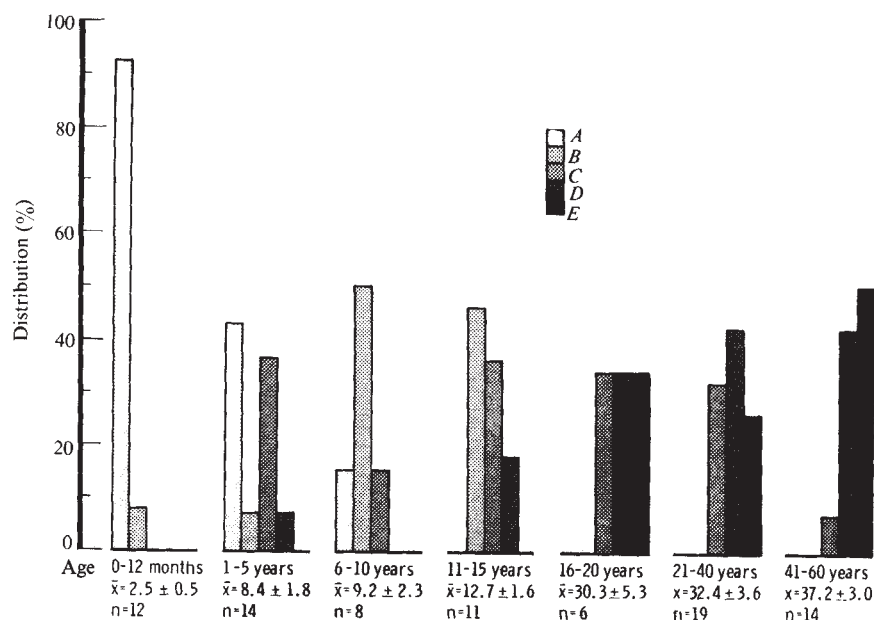
A striking feature is that during the first year of life, the levels of D β H are at the lower limits of sensitivity of the assay method. Screening of a series of laboratory animals of different species has also revealed a low circulating level of D β H. A common factor in all these subjects is the absence of erect posture. Prevention of postural hypotension is a function of the sympathetic nervous system¹². It is possible that the cir-

culating level of D β H remains low until an erect posture is assumed. It is also possible that the vesicular system which stores norepinephrine and D β H is not fully developed in infants. The continuing rise with age through late adulthood, however, cannot be explained in this way. A recent study of catechol-O-methyl transferase in liver has also revealed a progressive rise in concentration with age¹³.

function of the sympathetic nervous system in familial dysautonomia, it is probably not a primary feature of the disease^{15,16}. The possibility of a genetic influence other than that involved in familial dysautonomia must be considered.

Further studies on serum D β H activity levels in dysautonomia patients and in their parents as well as in patients with various neurological and mental disorders are now in progress.

Fig. 1 D β H activity in normal subjects of various age groups. Subjects were grouped according to enzyme activity expressed in nmol ml.⁻¹ serum. A, 0-5; B, 6-10; C, 11-20; D, 21-40; E, >41. $\bar{x}$ = Mean $\pm$ s.e.; n = number of subjects. Ninety subjects were studied, drawn from the in-patient and out-patient population and from laboratory personnel. Excluded were subjects with neurological diseases, malignancies and those on uncommon medications. There were no apparent differences in serum D β H activity between males and females.



Although the serum level of D β H among normal human subjects exhibits a broad range, each individual seems to maintain a relatively constant level. Two to four assays were done on fourteen individuals for a total of forty determinations. The average range of variation was $\pm 13\%$ with a maximum of $\pm 20\%$. It remains to be determined whether the levels can be influenced by exogenous factors such as strenuous physical exertion or prolonged immobilization, medications, extraneous illnesses and emotional stress. Longitudinal studies might show whether the pattern of development of circulating D β H correlates with other manifestations of sympathetic activity such as blood pressure levels.

Our results indicate that serum D β H activities can only be used for clinical purposes when the values are compared with control subjects of the same age group. In the course of these studies we have observed that two patients with pheochromocytoma tumours have circulating D β H levels within the normal range for their age groups while two children with neuroblastoma tumours had serum D β H activities approximately ten times higher than the mean value of control children of their age group. Mice bearing neuroblastoma tumours also have a significantly higher serum D β H activity value compared with the values of control mice¹⁴. These findings are compatible with other studies^{5,6} which indicate that most of serum D β H originates from the extramedullary tissue.

We have also investigated the serum D β H activity levels in children with dysautonomia and in some of their parents. The activity was either not detectable or very low in five of the fifteen children with the disease. The children with low serum D β H activity had, with one exception, at least one parent with the serum D β H activity lower than the mean value of the appropriate control subjects. Though the number of analysed sera is small, there is a clear suggestion of familial trend. In a recent independent study it was also shown that about 30% of patients with dysautonomia had no detectable or markedly decreased serum D β H activity levels and that the mothers of these patients also had decreased D β H activity levels (R. M. Weinshilboum and J. Axelrod, personal communication). Although there is considerable evidence for derangement of

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HL-A Antigens and Heteromorphic Fluorescence Characters of Chromosomes in Prenatal Paternity Investigation

We report here an investigation for legal abortion in which disputed paternity was resolved after a study of the HL-A antigens and the quinacrine fluorescence pattern of chromosomes in cultured amniotic cells.

A 27-year-old pregnant white woman married to a negro had had sexual intercourse with her husband as well as with a white man near the time of conception. This was her second pregnancy; the first one was terminated by legal abortion. The couple had decided to apply for another legal abortion and a divorce unless it could be shown likely that the husband was the father of the foetus. Their main reason for applying for an abortion was a social one, because it would always be understood that the husband could not be the father of a white child. A legal abortion would be permitted in Sweden under these circumstances. Therefore, amniocentesis was made, 138 days after the first day of the last menstruation. Cultivation of amniotic fluid cells was performed as described elsewhere⁴, and quinacrine fluorescence analysis of chromosomes was made on the amniotic cells after 16 days of culture (first subculture) and on cultured lymphocytes from the woman and the two men¹.

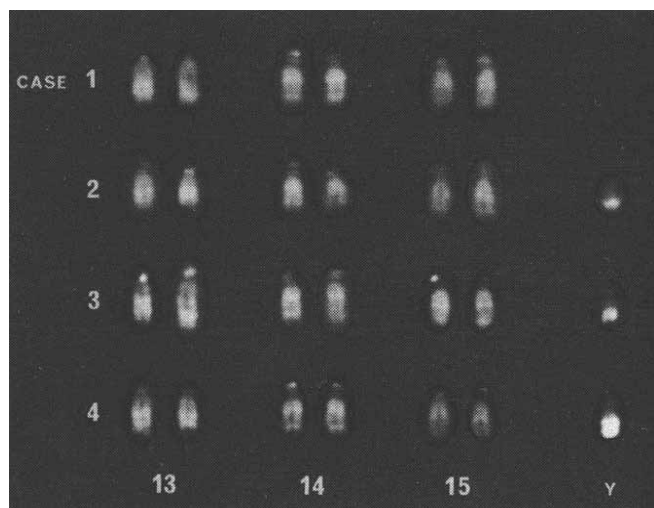


Fig. 1 Chromosomes 13, 14, 15 and the Y chromosome from the mother (1), the foetus (2), the husband (3) and the second man (4). Note the intensely fluorescent heteromorphic region on one chromosome number 13 as well as the similarity in size of the intensely fluorescent region of the Y chromosome in the foetus and the negro man.

The relative size of the Y chromosome of the foetus, which had a 46,XY chromosome constitution, and that of the two men was similar. The size of the intensely fluorescent part of the Y chromosome of the foetus and the husband was 33.2 ± 2.4 and $34.3 \pm 3.8\%$ of the whole Y respectively, while that of the other man was $50.4 \pm 3.5\%$ (Fig. 1). Six cells were measured in each case on photographic negatives. In addition, the foetal cells and those of the husband demonstrated an intensely fluorescent region on chromosome number 13 close to the centromere which was not seen in the cells from the woman and the other man (Fig. 1). No other heteromorphic fluorescent characters turned out to be informative in the present case.

Amniotic fluid cells in culture were tested for HL-A on four occasions, once in the first passage, once in the second, and twice in the third passage after a total of 22, 23, 26 and 27 days of growth, respectively. The same set of HL-A antisera was used as the one for lymphocytotoxicity (see below). A slight modification of the fibroblast-cytotoxic technique of Thorsby

and Lie⁵ was used. The relevant reaction patterns are given in Table 1. In spite of a rather high percentage of dead cells in the controls, it was possible to distinguish positive from negative reactions. The reliability of these results was substantiated by the facts that the presence of most antigens was tested by two different sera (or set of sera) and that the same reaction pattern was recorded on four different occasions.

Table 1 Reactions of the Amniotic Fluid Cells with Relevant HL-A Allo Antisera

Serum specificity	% killed cells
Anti-HL-A2	75-100
Anti-HL-A2, Ba* (=W28)	75-100
Anti-Ba* (=W28)	50- 75
Anti-HL-A5	< 15
Anti-HL-A5, R* (=W5)	50- 75

HL-A typing of the mother and the two men was carried out according to the lymphocytotoxicity test², and some of the antigens were checked on platelets using complement fixation. Most of the fifty-four lymphocytotoxic antisera used have been described previously^{3,6}.

The interpretation of the phenotypes and genotypes according to the results of HL-A typing of the amniotic cells of the foetus and the lymphocytes from the individuals involved appears from Table 2.

Table 2 Results of HL-A Typing

	Cells used for typing	Phenotypes	Most probable genotypes
Husband	Lymphocytes	HL-A2, Ba*(W28), R*(W5), MaPi(W17)	Ba*, R*/2, MaPi
Mother	Lymphocytes	HL-A1, 2, 8	2, X ₁ /1, 8
Foetus	Amniotic cells	HL-A2, Ba*, R*	Ba*, R*/2, X ₁
Second man	Lymphocytes	HL-A2, 7, ET*	?

W code refers to workshop numbers (Allen *et al.*, 1970). X₁ and X₂ indicate either homozygosity or an unknown gene present in the husband and the mother, respectively.

It seems that the result is only consistent with the fatherhood of the husband, as he, but neither the other man nor the mother, possesses the R* (=W5) antigen found in the foetus, and as there was no indication for the presence of any of the second man's antigens (HL-A7 and ET*) in the foetus.

It should be added that there is a slight possibility that the husband's genotype could have been 2, R*/Ba*, MaPi and that of the foetus 2, R*/X₁, as the two anti-Ba* sera used contained a very weak anti-HL-A2 component, which sometimes gives weakly positive reactions with cells being HL-A2 homozygous. This possibility seems rather unlikely, however, and it does not affect the conclusion as to the paternity.

The PGM system was also studied in leucocytes from the two men and the mother, as well as in cultures of cells from the amniotic fluid; but this system was not informative.

Thus, two heteromorphic fluorescent characters as well as the HL-A antigenic constitution indicated that the husband was the father of the foetus. This could be verified after the birth of the child, which was coloured. This study shows that the characters used can be successfully applied in prenatal genetic studies.

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Structure of Retinal Photoreceptor Membranes

IN an X-ray diffraction study¹ of the disk membranes of the frog, we derived the profile of average electron density along a line perpendicular to the membrane plane. It was assumed that the membrane consisted basically of a phospholipid bilayer, and a set of phases in agreement with the bilayer structure was adopted for the X-ray reflexions. Our results were consistent with, and they helped to define, the bilayer, but they did not exclude the possibility that the structure of the membrane might be other than a bilayer. We have now checked the correctness of our assumption and in effect have made a direct determination of the structure by means of X-ray diffraction alone.

A standard method of X-ray analysis was used to test the correctness of phase assignment. It is generally accepted that the phases are correct if, when a structure is swollen or shrunk, the calculated profile shows that the distance between the structure units changes but that the profile of the unit itself does not change. The result of the structure analysis then has not only a high degree of uniqueness but is physically reasonable. We have swollen and shrunk the array of disk membranes¹ by placing dissected retinæ in various concentrations or dilutions of ice-cold glucose-Ringer solution. The distance between the centres of the two membranes in a disk, and the distance D from the centre of one disk to the next, increased or decreased roughly in proportion as D varied from 230 Å to 370 Å. It is significant that the electron density profile for a single membrane of the disk remained, within experimental error, unaltered (Fig. 1). Thus the correctness of the phases used in calculating the disk profile is confirmed. We note that osmotic experiments similar to our own¹ have since been undertaken by J. M. Corless and his observations² confirm our own.

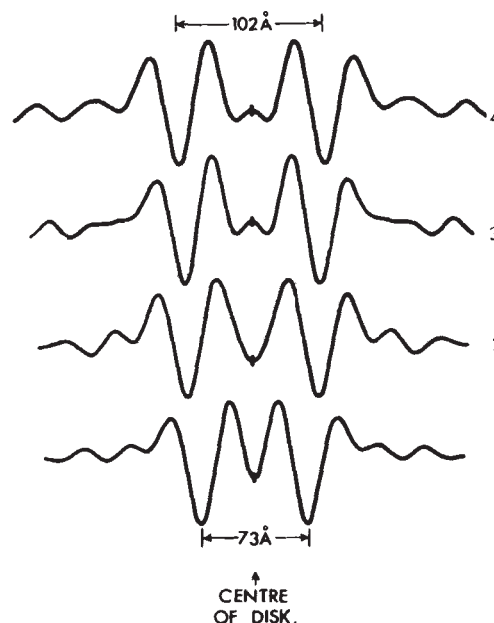


Fig. 1 Changing dimensions of the retinal rod disks with osmotic strength. As the osmotic strength of the glucose-Ringer solution decreases (from 1 to 4) the fluid space at the centre of the disk increases. At the same time, the single membrane profile to each side of the centre of the disk remains unchanged. Owing to special features of the diffraction data, the reliability of the asymmetry seen in the membrane profiles is in doubt. 1, In 0.5 M sucrose, $D=293$ Å; 2, in normal glucose-Ringer solution, $D=299$ Å; 3, in 0.90 glucose-Ringer solution, $D=311$ Å; 4, in 0.68 glucose-Ringer solution, $D=332$ Å.

The fact that the space inside the disk increased when the visual cell swelled confirms that the interior of the disk does not connect with extracellular space: if there were a connexion, one would expect the disks to collapse when the cells swell. After bleaching, changes in the electron density profile of the pair of membranes were within experimental error. An increase of 5 Å in D , however, appeared to be significant⁵.

In the membrane profile (Fig. 1) the peaks of electron density at the two sides of the bilayer are 40 Å apart, centre to centre, whereas the quantity of lipid present corresponds to an unhydrated bilayer 26 Å thick¹ and a peak-to-peak distance

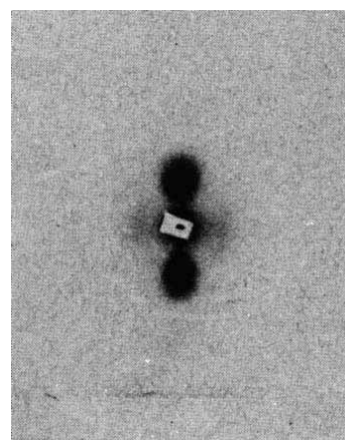


Fig. 2 The reflexion identified with the packing of rhodopsin molecules in the disk membrane. On the vertical axis through the centre of the pattern, the meridian, is seen (overexposed) disk profile diffraction. Centred on the horizontal axis, the equator, is a pair of diffuse reflexions indicating a close but irregular packing, in the plane of the membrane, of large molecules presumed to be rhodopsin. Diffraction from isolated disks has been identified by Blasie *et al.*⁴ as having similar origin.

of 22–28 Å. We therefore suggested¹ that the peaks corresponded not only to phospholipid head groups but also to thin layers of protein lying outside the bilayer, in contact with the head groups. Later consideration of a higher resolution profile and of profile diffraction from other membranes³, however, has led to a model with two narrow layers of lipid head groups 40 Å apart. This model clearly requires that a considerable part of the membrane protein penetrates into the lipid bilayer³. The higher resolution profile gives an indication of rhodopsin molecules laying partly in the bilayer structure. A pair of visible diffuse reflexions¹, which have peak intensity at a Bragg's-law spacing of about 55 Å on the equator, and extend above and below it (Fig. 2), are believed further to define the arrangement of the rhodopsin in the membrane.

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Isolation of Living Cells from Mature Mammalian Bone

LIVING cells have been isolated from articular cartilage of adult mammals by successive digestion with appropriate proteolytic enzymes to remove the matrix¹. Viability was checked by phase contrast microscopy; the chondrocytes were

motile at 37° C. Eosin dye-exclusion tests were compared with uptake of the acridine orange dye, 'Euchrysin 3R' as seen by fluorescence microscopy. The conclusion was that the vivid green fluorescence of nuclei and the red fluorescence of cytoplasmic granules in intact cells gave an excellent indication of the viability of the chondrocytes. It tallies well with other preliminary tests of viability, and was quicker and easier to perform. Fluorescence microscopy using 'Euchrysin 3R' had previously given a good indication of the viability of other mammalian cells². Other evidence that the chondrocytes were alive after isolation by digestion of the matrix was provided by allografting³ and by cultivation *in vitro*⁴. Isolated living chondrocytes have also been used for immunological typing⁵.

Recently techniques have been developed at the Institute of Orthopaedics for allografting articular cartilage with varying amounts of underlying bone⁶. Members of our own group have undertaken certain aspects of this work. Our first task was to develop viability tests for adult compact and cancellous bone. We decided to attempt the isolation of living osteocytes from adult bone.

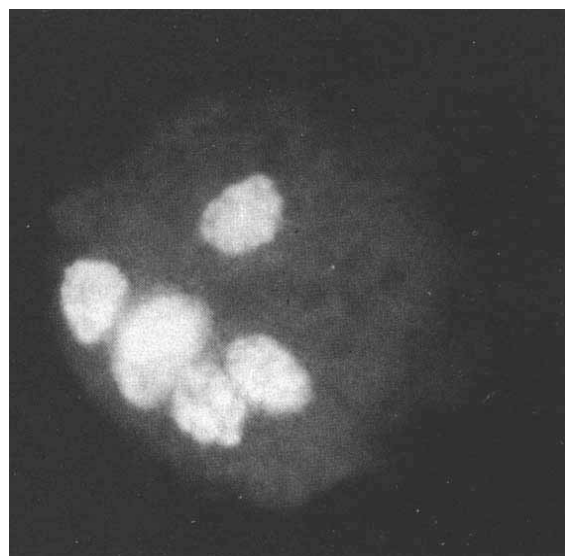


Fig. 2 A multinucleated cell isolated from a slice of human cancellous bone, mounted on agar and stained with 'Euchrysin 3R'. Transmitted fluorescent lighting ($\times 640$).

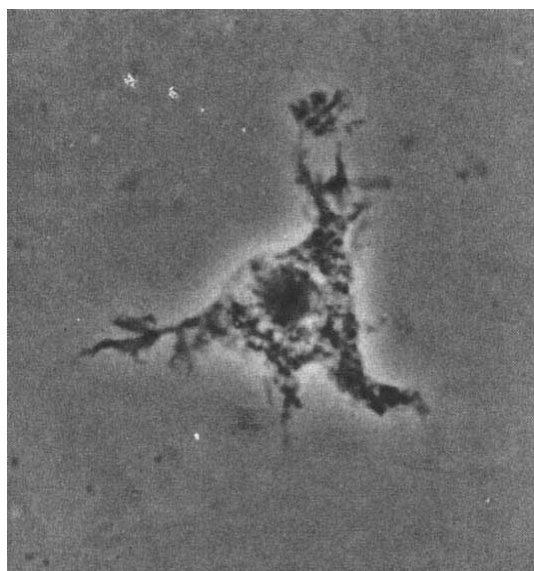
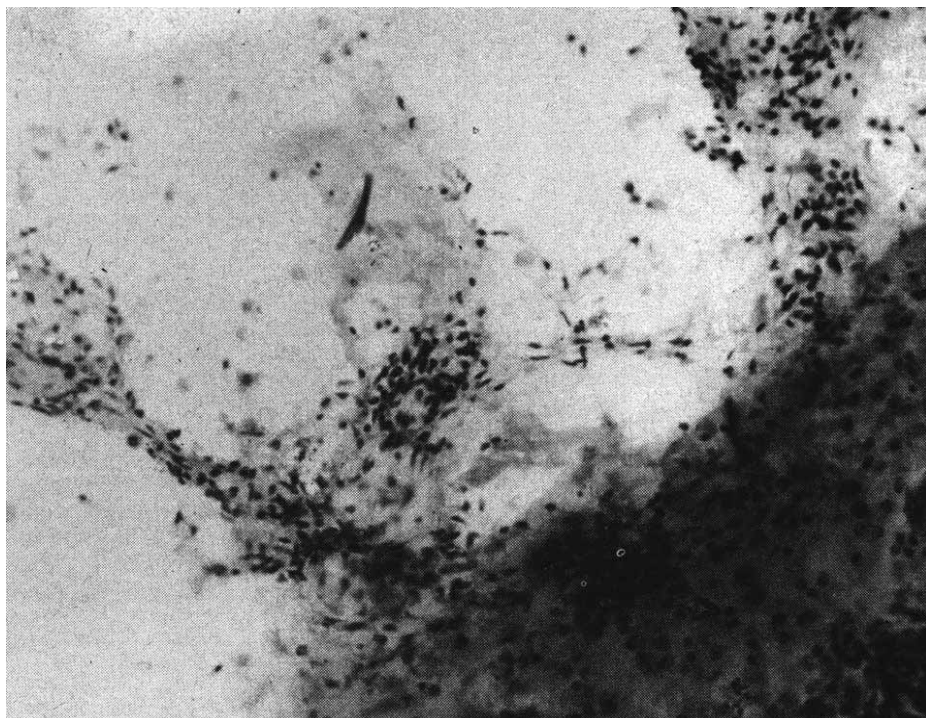


Fig. 1 A typical cell isolated from a slice of human cancellous bone, mounted on an agar coated slide and compressed slightly. Phase contrast ($\times 1,500$).

Living cells have been isolated from the calvaria and long bones of foetal mammals by digestion with collagenase⁷; at this stage of development the bones are not fully calcified. Digestion of mature bone is complicated by the presence of calcium salts. The evidence suggests that they are situated partly outside and surrounding the individual collagen fibrils^{8–10} and we therefore decided to remove these mineral salts first.

Slices of bone were cut from vertical segments of femoral heads or condyles of goat, pig, cow, dog, horse and man, using the Metals Research macrotome II fitted with a sintered diamond blade. Specimens from goats and pigs were sectioned within 2–3 h of death, while other bones were received 24 h after death. The human femoral heads had been removed in surgical operations 24 h previously. During cutting, the blade was lubricated and cooled by continuous irrigation with physiological medium. The slices were rigid and approximately 1.5 cm² and 100 μ m thick, weighing 15–20 mg. The rim of articular cartilage was excised. The slices were then dipped into distilled water for 2 s to disrupt the haematopoietic and other cells in the marrow cavities without damaging the cells within the bony spicules. Vertical slices were also cut from segments of compact bone from femoral shafts. The periosteal surface was excised but the distilled water rinse was unneces-

Fig. 3 A sample from a 7 day culture of cells isolated from goat cancellous bone, fixed and stained with haematoxylin and eosin ($\times 250$).



sary because of the absence of marrow. Samples from some slices were stained with 'Euchrysin 3R' and examined by fluorescence microscopy using incident illumination. Osteocytes could be clearly seen within lacunae and fluoresced green. The slices were transferred to conical flasks with 10 ml. of culture medium (TC 199) containing the dipotassium salt of ethylenediaminetetraacetic acid (EDTA) in a concentration of 1.5% for cancellous bone and 3.0% for compact bone. The pH was adjusted to 6.8, and the flasks were gassed with 5% CO₂ in air, then stoppered, sealed and shaken in a water bath at 38° C for about 14 h. The limp, decalcified slices were washed three times and then resuspended in 10 ml. of TC 199 containing 2.5 mg of collagenase (Sigma Type I, 125–200 U/mg). They were shaken for 2–3 h at 38° C until they had completely disintegrated. The turbid medium was centrifuged at 700–800g for 5 min and the deposit was washed and recentrifuged. The instruments, glassware and solutions used had been sterilized and aseptic precautions were taken throughout these procedures.

Samples of the deposit were mounted on agar coated slides, stained with 'Euchrysin 3R' and examined by phase contrast and fluorescence microscopy, using transmitted illumination. In spite of a certain amount of granular debris, many intact cells could be seen. They varied in size and shape, particularly in the deposits from cancellous bone. Phase contrast microscopy showed that many of the cells had several branched processes in different planes. They were difficult to photograph except after compression (Fig. 1). Cytoplasmic granules also showed up well by phase contrast microscopy and surrounded the nuclei, thereby tending to obscure their outlines. When seen by fluorescence microscopy the nuclei appeared bright green with sharp outlines; in the slender cells they were elongated, but in the plumper cells they were oval. A few cytoplasmic granules which fluoresced red could be seen, but they faded rapidly in ultraviolet light. The cells were similar in size and shape to the cells already seen in the undigested slices by incident fluorescent illumination. Small numbers of large multinucleated cells, possibly osteoclasts, were also seen in deposits from fresh cancellous bone (Fig. 2). Other deposits were used for eosin exclusion tests: 50–75% of the cells appeared to be alive. Smears were fixed and stained with haematoxylin and eosin. When slices of bone had been killed by boiling or by freezing and thawing, the osteocytes did not fluoresce and only a few fragmented cells were isolated by the technique described.

The differences between live and dead bone were striking.

Cultures of cells isolated from goat and human bone were set up in Petri dishes using TC 199, fortified by the addition of 10 ml. heated calf serum (Wellcome Type I) and 50 mg glycine, 20 mg proline and 10 mg glutamine per 100 ml. They were left to settle for 48 h and thereafter the medium was changed daily. After 7 days, collections of cells with long processes were adhering to the base of the dishes. Samples were removed for microscopical studies (Fig. 3). Quantitative experiments and biochemical studies with these cells are now under way.

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Obituary

Dr R. L. Knight

DR R. L. KNIGHT, head of the Fruit Breeding Section of East Malling Research Station, died suddenly at his home on February 15, only a few weeks before he was due to retire.

Robert Knight, like many others who have made great contributions to agriculture, started his career at Wye College. There he took a Diploma in Agriculture, and went on to spend a year each at St John's College, Cambridge, and at the Imperial College of Tropical Agriculture, Trinidad. In 1929 he was recruited to the staff of the Empire Growing Corporation, as it then was, and seconded to the Sudan where, in close collaboration with the Department of Agriculture, and with colleagues of the Corporation staff in other territories, he made a great contribution to the future of both Sudan and other African territories.

At the time Sudan's future lay in the long-term success and stability of the great Gezira irrigation scheme which was to provide not only food but also cash in the form of cotton for export to Lancashire where Empire-grown cottons capable of rivalling the best American selections were needed. Existing cotton strains suffered, often disastrously, from "Black-Arm disease" due to *Bacterium malvacearum*. Though some genetic resistance had been recognized and used, the protection afforded was both inadequate and unreliable. Official policy based all hopes of limiting the disease on controlled rotation and on the plant sanitation procedures introduced by Massey. Nevertheless, crippling attacks occurred periodically. To the vulnerable Egyptian cottons as to the Upland cottons, Knight laboriously transferred genes and sup-

porting genes for resistance which he had identified in the course of detailed research among diverse cottons, often of little or no commercial value. In so doing he was well ahead of his time, demonstrating the great ecological importance of multiple resistance factors and the possibility of combining high commercial values with resistance effective over a wide range of environmental conditions.

Knight insisted on remaining at work under the relatively primitive conditions at Shambat and when Hutchinson, already a world authority on the taxonomy and genetics of the cottons, arrived there, bringing with him the Corporation's World Collection of Cottons, Knight overbore all more cautious councils, and insisted on subjecting the collection to massive overall infection. From the results and from the belief of Nye and Hutchinson that additional resistance factors would be found among the Upland cottons, there emerged the Albar cottons and their derivatives which, by combined high resistance and commercial value, have been of such benefit in Africa. The value of a reliable cash crop that can be alternated with an acceptable food crop needs no emphasis.

Recognition of his work led to his appointment as Chief Geneticist of the Sudan Government and, in 1952, to the award of the OBE.

His chief work in the Sudan completed, Knight returned to England and in 1954 accepted an equally challenging post at East Malling. Despite the successes of Crane and Grubb, the application of genetic analysis to fruit breeding had in general progressed little beyond selection from the progeny of existing cultivars. Nevertheless, at East Malling the foundations had already

been laid for the far more ambitious programmes which Knight pursued in the eighteen years following his appointment. At the National Fruit Trials at Faversham in Kent, Knight had available basic breeding material and a unique collection of cultivars of hardy fruits from all over the world. Knight inspired his team in all aspects of their work, and aroused the interest of his colleagues in pathology and entomology, while the use he made of screening techniques on the varietal collections was very similar to his use of them during his earlier studies on cotton.

His successes at East Malling are summed in the success of his team in producing, in less than 20 years, a wide range of defined genetic resources in all the tree and bush fruit crops with the exception of cherries. Progress in the planned combination of desired characters has been so substantial that clones derived from seedlings are already under test as potential replacements for existing commercial cultivars of bush fruits while the number of clones of tree fruit requiring similar tests is so large that careful advance planning is needed to ensure their speedy evaluation.

East Malling, and plant breeding as a whole, are the poorer for Knight's death. The help he gave to others, whether in the form of basic breeding material, in the editing of the scientific writings of others, or in his outstanding series of abstract volumes on the genetics of cotton, and of tree and bush fruit crops, is greatly valued. Characteristically, he was planning to use his retirement in even more generous assistance to others; his many friends, and many who will never know his name, have good cause to be grateful for his life and work.

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